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In this Issue

Military Training and Principles of Learning: ROBERT M. GAGNÉ	83
Psychology and the Space Frontier: WALTER F. GREYER	92
Contrasting Systems of Work Organization: RAYMOND A. KATZELL	102
Image of Industrial Psychology among Corporate Executives: M. R. FEINBERG AND J. LEFKOWITZ	109
Comment	112
Psychology in the News	115
Psychology in the States	117
Notes and News	120
Convention Calendar	126
Convention Forms: 1962 APA Annual Convention	127

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MILITARY TRAINING AND PRINCIPLES OF LEARNING^{1,2}

ROBERT M. GAGNÉ

Princeton University

THE subject chosen for this address is one which I think I can view with a certain perspective. Stated very briefly, this circumstance arises from the fact of my changes in occupation, from that of an investigator of learning principles in academic laboratories, to a research administrator of programs of military training research in government laboratories, and back again to an academic laboratory. In making the remarks to follow, I claim nothing more than this perspective, which perhaps carries with it a certain detachment, or freedom from involvement with particular learning theory, as well as with particular training problems.

What I should like to talk about are some general impressions concerning the applicability of learning principles to military training. In the time available to me, I cannot really do more than this. It would be satisfying to think that I could review and marshal the evidence from military studies of training in a truly systematic manner. I am not sure that this can be done even with unlimited time. But at any rate, my aim is much more limited than this. Perhaps it can be stated in the following way. Suppose that I were a learning psychologist, fresh out of an academic laboratory, who was to take a new job in charge of a program of research on some type of military training. What principles of learning would I look for to bring to bear on training problems? What kinds of generalizations from laboratory studies of learning would I search for and attempt to make use of in training situations? The answers I shall suggest for these questions require first a consideration of what kinds of principles have been tried, and how they have fared.

Some Representative Military Tasks

First, we need to have in mind certain representative military tasks for which training either is or

has been given, in order that we can consider in detail the kinds of learning principles that are applicable. Here are three which will serve well as examples: (1) flexible gunnery; (2) putting a radar set into operation; (3) finding malfunctions in an electronic system.

Flexible gunnery. The gunner of a now obsolete type of bomber aircraft was typically located in the waist or the tail of the plane, and aimed and fired a gun at fighter aircraft attacking on what was called a "pursuit course." To do this he looked at the attacking fighter through a reticle containing a central dot, which he lined up with the target by rotating his gunsight horizontally and vertically. At the same time, he had to "frame" the aircraft within a set of dots arranged in a circle whose circumference could be varied by rotating the round hand-grip by means of which he grasped the gunsight. This is the kind of task the psychologist calls "tracking," on which a great many laboratory studies have been carried out. It was, of course, tracking simultaneously in the three dimensions of azimuth, elevation, and range. To perform this task, the individual had to learn a motor skill.

Putting a radar set in operation. This kind of task is typically what is called a "fixed procedure." That is, the individual is required to push buttons, turn switches, and so on, in a particular sequence. Here, for example, is a set of steps in a procedure used by radar operators to check the transmitter power and frequency of an airborne radar (Briggs & Morrison, 1956):

1. Turn the radar set to "Stand-by" operation
2. Connect power cord of the TS-147
3. Turn power switch on
4. Turn the test switch to transmit position
5. Turn DBM dial fully counter-clockwise
6. Connect an RF cable to the RF jack on the TS-147

There are 14 more steps in this procedure. Notice that each of the steps by itself is easy enough; the individual is quite capable of turning a switch or connecting a cable. What he must learn to do, however, is to perform each step in the proper sequence. The sequence is important, and doing step 5 before step 4 may be not only an

¹ Presidential address delivered at the annual meeting of the Division of Military Psychology, 69th Annual Convention of the American Psychological Association, New York, N. Y., September 5, 1961.

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error, it may be dangerous. What must be learned, then, is a sequence of acts in the proper order.

Finding malfunctions in complex equipment. This is in many respects a most complex kind of behavior. There are of course some very simple kinds of equipment in which this activity can be reduced to a procedure; and when this is true, the task is one that can be learned from that point of view. But the major job, for complex equipment, is one of troubleshooting, a problem-solving activity that has considerable formal resemblance to medical as well as other kinds of diagnosis. Suppose this is a radar set, again, and that the initial difficulty (symptom) is that no "range sweep" appears on the oscilloscope tube face. Beginning at this point, the troubleshooter must track down a malfunctioning component. He does this first by making a decision as to how he will check the operation of subordinate parts of the system, next by carrying out the check and noting the information it yields, next by making another decision about a next check, and so on through a whole series of stages until he finds the malfunctioning unit. In each of these stages, he presumably must be formulating hypotheses which affect his actions at the next stage, in the typical and classically described manner of problem solving. What does the individual have to learn in order to solve such problems? This is indeed a difficult question to answer, but the best guess seems to be that he must acquire concepts, principles, rules, or something of that nature which he can arouse within himself at the proper moment and which guide his behavior in diagnosing malfunctions.

Here are, then, three types of activities that are not untypical of military jobs, and which are aimed at in military training: a motor skill like flexible gunnery; a procedure like putting a radar set into operation; and troubleshooting, the diagnosing of malfunctions in complex electronic equipment. Each one of these tasks has been examined more or less intensively by military psychologists and learning specialists. Among other things, each of these tasks can be shown to be not entirely unique, but to represent a rather broad class of tasks, in its formal characteristics, which cuts across particular content or occupational areas. For example, flexible gunnery is a tracking skill, which formally resembles many others, like maneuvering an airplane, sewing a seam on a sewing

machine, hovering a helicopter, and many others. As for procedures, these are common indeed, and may be found in jobs such as that of a clerk in filling in or filing forms, a cook preparing food, or a pilot preflighting an airplane. Diagnosing difficulties is certainly a widely occurring kind of activity, which may be engaged in by the leader of a group who detects the symptom of low morale, as well as by a variety of mechanics who "fix" equipment of all sorts. Accordingly, one should probably not consider these particular examples as peculiar ones; instead, they appear to be representative of a wide variety of human activities.

Learning

How are these three kinds of tasks learned? What is it that the learning psychologist can say about them which will enable anyone (the teacher, the curriculum builder, the training manager) to undertake to arrange the external conditions in such a way that the desired performances will be acquired with the minimal expenditure of time, money, and wasted effort?

Suppose that you were, in fact, a psychologist who had studied learning, both animal and human, from the standpoint of experiment and theory, and that you were faced with this problem. How can scientific knowledge of learning be used to improve the process of training? Notice how I have stated this question. I am not asking, how can a scientific approach be applied to the study of training? Nor am I asking, how can experimental methodology be applied to the study of training? There are certainly answers to these questions, which have been provided by several people, notably Crawford (1962). The question is, rather, how can what you know about learning *as an event*, or *as a process*, be put to use in designing training so that it will be maximally effective?

The psychologist who is confronted with this question is likely to appeal, first, to a basic point of view towards learning which is so highly ingrained it may be called an *assumption*. Beyond this, and secondly, he looks for certain *principles* which have been well established by experiment. These are principles which relate certain variables in the learning situation, like time intervals between trials, sequence of trials, kind of feedback after each trial, and so on, to such dependent

variables as rate of learning or goodness of performance. Let us try to see what can be done both with the basic assumption and with some of the more important of the principles.

The assumption. The assumption that many learning psychologists would bring to the problem of designing training is something like this: "The best way to learn a performance is to practice that performance." I should like to show, later on, that this assumption is by no means a good one. But before I do that, I want to consider the question, where does this assumption come from, anyhow? First, it seems to have a cultural basis, by derivation from the writings of John Dewey, preserved in the educational catch-phrase "learning by doing." Second, it appears to come by unwarranted generalization from laboratory prototypes of learning such as the conditioned response. In conditioning, classical or otherwise, one observes learning only *after* the animal has made the first *response*. Thus, performance comes first, and learning is often considered to result from practice of this performance. Third, the assumption comes from theory which deals with conditioning, and which conceives of what is learned as either a response or an association terminating in a response, in either case established by *practicing the response* (with reinforcement). Without going into the matter further at the moment, the basic reason that generalization of this notion to the learning of the human tasks I have mentioned seems questionable is simply that the responses required (turning switches, inserting plugs, moving handles) do not have to be learned at all—they are already there in the human's repertoire.

Principles

Beyond this assumption that learning comes about when performances are practiced, what *principles* can the learning psychologist depend on? What kinds of conditions have been found to affect the rate of learning? What findings can he bring to bear on the problem of designing training to be maximally effective?

Let me mention some of the best-known of these principles, not necessarily all of them, using various sources. In part, I shall depend on an excellent article by Underwood (1959). First of all, there is *reinforcement*, the principle that learn-

ing will be more rapid the greater the amount of reinforcement given during practice. Other principles include *distribution of practice*, *meaningfulness*, increasing the *distinctiveness* of the elements of a task, and *response availability*.

These principles would appear to provide the learning psychologist with a fairly adequate bag of tricks with which he can approach the job of designing effective training. There is much evidence in the experimental literature that one can in fact alter the rate of learning by manipulating these variables in the learning situation, whether one is working with single conditioned responses or with verbal material having a somewhat more complex organization. Each of these variables, so far as is known, can be manipulated to make a dependable difference on learning, in the direction of increased as well as decreased effectiveness.

Using these Assumptions and Principles in Training Design

How does one fare if he seriously attempts to use this basic assumption and these principles to design effective training situations? *Not particularly well.* The assumption that the most effective learning is provided by practice on the final task leads one astray on many occasions. As for the principles, sometimes they can clearly not be applied, that is, there is no way to manipulate the training situation in the manner suggested by the principle. In other instances, the evidence simply fails to support the principle. When this happens, there may be good theoretical reasons for the event, but this still does not restore one's faith in the usefulness of the principle.

It will be possible here only to give a few examples of military training situations in which these assumptions and principles failed to work, but I have chosen them to be as representative as possible. Let me emphasize again that I do not maintain that these examples demonstrate that the principles are invalid. I simply want to show that they are strikingly inadequate to handle the job of designing effective training situations.

Motor skill. First let's consider what is perhaps the most difficult case, the learning of a motor skill like gunnery. What happens if we try to employ the assumption that the best way to learn gunnery is to practice gunnery? Using the kind of task required of a flexible gunner, a number of

studies were made of the conditions of learning for this performance. One of the earliest ones, during World War II, reported by Melton (1947), showed that different amounts of practice in firing at sleeve targets during one through ten gun-camera missions made no significant difference in the measured proficiency of gunners. A number of other studies of gunnery also indicate the very small and often insignificant effects of practice continued beyond the first three trials or so (Rittenhouse & Goldstein, 1954). Furthermore, several such studies confirm the finding that the major improvement in this performance comes as a result of informing the learners of the correct picture to be achieved in ranging (i.e., so that the dots just touch the wing tips of the target aircraft) (Goldstein & Ellis, 1956). In other words, to summarize the finding very briefly, the evidence is that simple practice on the gunnery task is not a particularly effective training method; instructions about the correct sighting picture for ranging is much more effective in bringing about improved performance. Perhaps there are good theoretical reasons for this. But the fact remains that practicing the performance is *not* the best way to learn.

What about the principles of learning? Well, let's consider the one which a learning psychologist might be inclined to think of first—reinforcement, or the introduction of knowledge of results during practice. Translated into a form applicable to motor skills learning, the principle is that the more adequate are the knowledge of results, the more rapid the learning. This variable, too, has been tried out in a number of studies. Typically what was done was to augment the knowledge of results that came to the gunner through his observing his own tracking performance on a screen, by providing an extra cue, such as a buzzer, which sounded whenever the gunner was exactly on target in all three dimensions. The effect of this extra cue, it was found, was to improve the performance during learning. But did this mean that the learning itself was more effective, or simply that the buzzer "propped up" the performance? One seeks the answer to this question by comparing the performance of buzzer-trained and non-buzzer-trained groups on a standard criterion task without the buzzer. When this was done, the findings in several studies were negative (cf. Goldstein & Ellis, 1956), and one (Goldstein & Rittenhouse,

1954) actually showed that learners who had the advantage of augmented knowledge of results (reinforcement) exhibited a lower performance on a second gunnery task.

Other learning principles were unconfirmed in training situations. For example, a carefully executed study could find no evidence for changes in learning as a result of alterations in conditions of practice and rest periods (Rittenhouse & Goldstein, 1954). Still other variables simply cannot be used in the training situation. For example, the meaningfulness of the task is set by the task itself, and cannot be altered by changing the conditions of training. Similarly, the internal similarity of the elements of the task are fixed by the task; one cannot, for example, change the degree of resemblance of the aircraft or of the tracks they follow by simply redesigning the training, without setting about to change the nature of the task itself. (I omit here a discussion of the transfer effects of training with an easy discrimination to performance on a hard discrimination, and vice versa. This is a different principle than the one under discussion, and the evidence about it is not clear-cut.) What about response availability or familiarity? From the evidence on practice previously cited, as well as studies on part-training (cf. Goldstein & Ellis, 1956) it seems fairly clear that the responses in this task (turning knobs, moving the gunsight up and down with a handle) were highly familiar in the first place. No one, so far as I know, ever seriously proposed that they were not.

Perhaps these examples are sufficient to at least raise doubts about the usefulness of the learning psychologist's assumptions and principles, when he attempts to apply them to the practical job of designing training for motor skills. On the whole, it may fairly be said, I think, that the assumption was often wrong and the principles were seldom useful in bringing about training improvement. I caution you again that I am not saying the learning psychologist was unsuccessful in improving training. In many instances, he was very successful. What I am trying to answer is the question, when he was successful, what knowledge or set of principles was he using?

Procedures. There are not many analytical studies of the learning of procedures. Perhaps the reason for this is that learning procedures is

relatively such an easy matter, and the methods used to train them seem relatively so obvious, that little work was done on them. Consequently, I shall have to base my arguments primarily on these obvious features, rather than on a great deal of experimental evidence.

Suppose one is faced with the task of training someone to "turn on" a radar set by turning and pushing a series of fifteen switches in a particular sequence. (This is taken to be a simplified version of a representative procedural task.) How does one go about it? If one proposes to conduct training simply by "practicing the task" it becomes obvious almost immediately that this is an inefficient method to use. What is usually done is this: the learner is provided with a *list*, which states, in effect, "First, turn on power switch; second, depress voltage switch; third, set voltage knob to reading 10; etc." (e.g., Briggs & Morrison, 1956). Now the individual may be required to commit the list to memory first, and then proceed to the task; or, he may be allowed to use the list while he practices going through the sequence. The important thing is, however, that it is the *learning of the list* that contributes most to the performance of the task, not the practice of the switch-pressing responses, another example contrary to the principle that the best way to learn is to practice the required performance. I do not say that the performance should never be practiced, simply that something other than direct practice of the final task is more effective for learning procedures, just as is true for motor skills in the example previously described.

Learning principles applied to the training of procedures do not fare very well, either, although again I must note the absence of experimental evidence. One cannot alter meaningfulness, and in most cases the responses required are highly familiar. When they are not, as may be the case when a single step requires the use of an unfamiliar tool, this principle may actually have some limited usefulness. Sometimes the principle of increasing the distinctiveness of the elements of the task can be used, and one would indeed expect it to work. For example, one could put distinctive cues or labels on each of the switches in the 15-switch procedure, and this might be expected to speed up the rate of learning. However, it may be noted that this becomes a matter of changing the

task (i.e., the equipment), rather than of changing the conditions of learning. From evidence on the learning of nonsense-syllable lists, one would not expect a variable like distribution of practice to make much difference as a training variable, as Underwood (1959) has noted. Again a review of learning assumptions and principles has indicated limited usefulness.

Diagnosing malfunctions. When we turn to a consideration of troubleshooting complex equipment, even the most theoretically-dedicated learning psychologist is forced to recognize, almost from the start, that the idea of learning to troubleshoot by simply practicing troubleshooting verges on the ridiculous. The most obvious reason is that one cannot identify a single *task* to be practiced. The troubleshooter is faced with a great variety of initial problem situations, each of which may have a great variety of causes. He cannot possibly practice solving all of them. In fact, it is clear that he must learn not a single task, but a *class of tasks*, or perhaps even several classes of tasks. Yet people do learn to do them, quite successfully, without ever doing anything that can legitimately be called "practicing the final performance."

What they do learn, among other things, is an elaborate set of rules pertaining to the flow of signals through a complex circuit. To a large extent, they learn these rules by looking at and responding to a circuit diagram which is a representation of the equipment rather than the equipment itself. And they use the rules in thinking about the signal flow, that is to say, in making successive decisions leading to a solution of the problem (finding the malfunction).

Since, as I have said, it is impossible to define a single task to be practiced in learning troubleshooting, it is just about equally difficult to apply the principles of reinforcement, meaningfulness, internal differentiation, and so on, to the design of training. If one accepts the task of "learning the rules" as what must be done, it is of course possible to ask the question as to whether such learning variables would apply to that task. This is a job that may some day be done by those interested in research on "learning programming." But it has not been done as yet. The evidence to date (such as it is) has not indicated strong effects, or even significant ones, for the variable of

reinforcement in connection with learning programs (Goldbeck & Briggs, 1960). Other variables have not yet been investigated in relation to the learning of rules and principles.

What Is Applicable to the Design of Training?

Does this mean that the psychologist has virtually nothing to offer to the problem of designing effective training? Have the results of psychologists' efforts to improve training been entirely negative? Quite to the contrary, it seems to me that efforts can be identified which were quite effective in producing significant improvements in training, and which led to some demonstrably useful designs for training. But the principles which were found to be effective for such purposes were not those that have been mentioned.

Here are the psychological principles that seem to me to be useful in training:

1. Any human task may be analyzed into a set of component tasks which are quite distinct from each other in terms of the experimental operations needed to produce them.

2. These task components are mediators of the final task performance; that is, their presence insures positive transfer to a final performance, and their absence reduces such transfer to near zero.

3. The basic principles of training design consist of: (a) identifying the component tasks of a final performance; (b) insuring that each of these component tasks is fully achieved; and (c) arranging the total learning situation in a sequence which will insure optimal mediational effects from one component to another.

These statements certainly imply a set of principles which would have very different names from those we are now most familiar with. They are concerned with such things as *task analysis*, *intra-task transfer*, *component task achievement*, and *sequencing* as important variables in learning, and consequently in training. These principles are not set in opposition to the traditional principles of learning, such as reinforcement, differentiation of task elements, familiarity, and so on, and do not deny their relevance, only their *relative importance*. They are, however, in complete opposition to the previously mentioned assumption "the best way to learn a task is to practice the task."

It should also be pointed out here that I am

unable to refer to any well-organized body of experimental evidence for these newly proposed principles. They come instead by inference and generalization from a wide variety of instances of learning and military training. I do not claim more for them than this. But they have to be stated before any systematic experimental work can be done on them.

Let me try now to illustrate a definite meaning for these principles with some examples. Consider first the procedural task described previously. "1. Turn radar set to 'standby' operation; 2. Connect power cord of the TS-147; 3. Turn power switch on; 4. Turn test switch to transmit position; etc." The first step to be undertaken here is to analyze this task; and (with certain minor assumptions on our part), this is seen to be, first, the learning of an order series of responses to things; and second and subordinate to this, the locating of these things. These two *component tasks* have a hierarchical relationship to each other, and immediately suggest the proper *sequencing* for the arrangement of the learning (or training) situation. That is to say, what must first be undertaken is that the learner learn what and where the "things" are (the "standby operation" switch, the "TS-147," the power switch, the test switch, and so forth). This is a matter of identification learning, which has considerable resemblance to the paired-associate learning of the psychological laboratory. Having achieved this subordinate task, it is then possible for the learner to undertake the second, or "serial order of things" task. According to the principle proposed here, maximal positive transfer to this task would be predicted following completely adequate performance on the subordinate task of identifying the "things."

Laboratory experiments which have undertaken to test such a hypothesis seem to be scarce. It is possible, however, to make reference to two studies (Primoff, 1938; Young, 1959) which have some suggestive findings. Generally speaking, when one learns a set of paired associates first, and then undertakes the learning of these units serially, there is high positive transfer; but when one learns units serially first, the amount of transfer to paired associate learning is very low indeed. These results strongly suggest that there is a *more efficient* and a *less efficient* sequence which can be arranged

for the learning of a procedural task, and that this sequence involves learning one subtask before the total task is undertaken. A procedure is a task that can be analyzed into at least two component tasks, one of identification, and the other of serial ordering. The first is subordinate to the second in the sense that it mediates positive transfer to the second, provided it is first completely mastered.

Can this kind of analysis be applied to a more complex task like troubleshooting? Indeed it can, and those psychologists who thought about the problem of training troubleshooting came close to the kind of analysis I have suggested. Generally speaking, they recognized that troubleshooting some particular equipment as a final performance was supported by two broad classes of subordinate tasks. First, there was knowledge of the rules of signal flow in the system, and second, the proper use of test instruments in making checks. The rules of signal flow themselves constitute an elaborate hierarchy of subordinate tasks, if one wants to look at it that way. For example, if the signal with which the mechanic is concerned is the output of an amplifier, then it may be necessary that he know some of the rules about data flow through an amplifier. Thus the task may be progressively analyzed into subordinate components which support each other in the sense that they are predicted to mediate positive transfer.

The task of using test instruments in making checks provides an even clearer example, perhaps. Obviously, one subordinate task is "choosing the proper check to make" (presumably a matter of knowing some "rules"); another is "selecting the proper test instrument" (an identification task); still another is "setting up the test instrument" (a procedural task, which in its turn has components like those previously described); and another is "interpreting the instrument reading" (another task involving a "rule"). Even identifying these component tasks brings to troubleshooting a vast clarification of the requirements for training. If one is able to take another step of arranging the proper sequencing of these tasks in a training program, the difference which results is remarkable. This is the interpretation I should be inclined to make of the studies which have demonstrated significant improvements in troubleshooting training, such as those of Briggs and Besnard (1956); of Highland, Newman and Waller (1956); and of

French, Crowder, and Tucker (1956). In providing training which was demonstrably successful, these investigators were giving instruction on a carefully analyzed set of subordinate tasks, arranged in a sequence which, so far as they could tell, would best insure positive transfer to the variety of problem situations encountered in troubleshooting. It was the *identification of these tasks and this sequence* which I believe was the key to training improvement.

A good deal of other work also proceeded along these lines, although not always with a terminal phase of measured training effectiveness. For example, a whole series of studies by Miller and Folley, and their associates, were concerned with what was called *task analysis*. They had such titles as these: Line maintenance of the A-3A fire control system: III. Training characteristics (Folley & Miller, 1955); Job anticipation procedures applied to the K-1 system (Miller, Folley, & Smith, 1953); A comparison of job requirements for the line maintenance of two sets of electronic equipment (Miller, Folley, & Smith, 1954). What was all this talk about task analysis? Did it have anything to do with training? My answer is that it had to do with training more than with anything else. These were thoroughgoing and highly successful attempts to identify the variety of tasks contained in a job, and the variety of subtasks which contributed to each task. There was in fact explicit recognition of the idea that successful final performance must be a matter of attaining competence on these subtasks. So here again was the notion that effective training somehow depended on the identification of these subordinate tasks, as well as on their arrangement into a suitable sequence to insure positive transfer to the final performance.

A third source of these ideas in military training research should be mentioned. This was the development of training devices applicable to such jobs as electronic maintenance. It came to be recognized that these devices were in some respects very different from the traditional trainers such as those for developing skill in aircraft maneuvers. They were called "concept trainers", and this, as Briggs' (1959) discussion of them implies, was another name for "teaching machines." As such, they were developed independently of Skinner's ideas, and they were in fact based upon an

entirely different set of principles, as is clear from the accounts provided by Briggs (1956), Crowder (1957), and French (1956). Each of these training devices (or teaching machines), aside from its hardware engineering, was developed on the basis of a painstaking task analysis, which identified the subordinate tasks involved in a total task like troubleshooting a particular electronic system. The subordinate tasks thus identified were then incorporated into a sequence designed to insure maximal positive transfer to the final task. There were certainly some programing principles, but they bore little resemblance to those which are most frequently mentioned in recent literature; in my opinion, they were much more important than these.

Still a fourth area of effort in training research was related to these ideas. This was the development of techniques to provide behavioral guides, or "jobs aids" in support of performance in various technical jobs (Hoehn, Newman, Saltz, & Wulff, 1957). In order to do this, it was found necessary to distinguish between those kinds of capabilities which could best be established by thorough training, and those kinds which could be established by minimal training plus the provision of a check list or handbook. Obviously, here again there had to be a detailed task analysis. Subordinate tasks had to be identified which would mediate transfer either to the kind of performance required without a handbook, or the kind required with a handbook. Besides the initial task analysis, it is again evident that this line of work was making use of ideas about component task achievement and intratask transfer.

Summary

Now that I have conveyed the message, my summary can be quite brief. If I were faced with the problem of improving training, I should not look for much help from the well-known learning principles like reinforcement, distribution of practice, response familiarity, and so on. I should look instead at the technique of task analysis, and at the principles of component task achievement, intratask transfer, and the sequencing of subtask learning to find those ideas of greatest usefulness in the design of effective training. Someday, I hope, even the laboratory learning psychologist will know more about these principles.

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PSYCHOLOGY AND THE SPACE FRONTIER¹

WALTER F. GREETHER

Behavioral Sciences Laboratory, Wright-Patterson Air Force Base

IN the history of our world, few, if any, previous events have had an impact on science equal to the successful launching of vehicles into space. For many centuries people have looked up at the heavenly bodies and dreamed of the time when men could leave the earth and explore the universe. While, as yet, only the most simple beginnings have been made in terms of manned trips into space, the gravitational ice has been broken; manned flights to the moon and the planets now appear within man's grasp. The achievement of this age-old dream of flight into space had to wait until our technology, particularly the physical and engineering sciences, had prepared the necessary foundation of scientific knowledge. Many further advances in science and technology must be made before the major objectives of manned space flight become possible. So dependent is space flight on science that the people of all nations around our globe measure the scientific capability of our country and the USSR by our successes in space. That this is a valid measure we, as psychologists, might disagree, but it is used nevertheless.

Aside from being a milestone and a measure of our scientific progress, the beginning of the space age has had several important beneficial effects on science in general:

1. It has focused the spotlight of public attention on science, and thereby increased the financial support of many fields of science.
2. It has opened up scientific frontiers formerly not accessible for research and exploration.
3. It has created an urgent need for new scientific knowledge.
4. It has stimulated scientists into thinking about new problems, theories, and hypotheses.

All in all, I believe that the benefits to science have been and will continue to be very great and that much of the new knowledge will be beneficial in other fields of human endeavor quite unrelated to space flight.

¹ Presidential Address to Division 21 of the American Psychological Association, New York City, September 4, 1961.

While the impact of space flight on many areas of physical science and engineering are quite obvious, what has been the effect on psychology and how has psychology been contributing to the national space flight efforts?

My purpose in this paper is to examine some of the activities of psychologists who have been most directly involved in space flight work, to see what the nature of their contribution has been, and to look at some of the future problems that space flight poses for psychology. Before proceeding with this review, I should hasten to place some bounds around the work I will cover. First of all, I will, of necessity, pass over the work of psychologists, including many in this room, whose basic contributions laid the foundations for the psychologists directly involved in current space flight efforts. Also, since this is a gathering of the Society of Engineering Psychologists, I will concentrate on activities that we would consider to be in the domain of engineering psychology.

At this point it might be helpful to look back almost 60 years to the time of the Wright brothers and the first manned flight in an airplane. Certainly this was an event comparable in importance to the flight of Gagarin, Titov, Shepard, and Grissom into nearby space. I think it is safe to say that the Wright brothers accomplished their feat without assistance from the science of psychology. No doubt psychology could have contributed to aviation in those early years, but neither aviation nor psychology were ready for such a partnership. Except for some contributions to selection of aviators during World War I, psychology and aviation pretty much went their separate ways until the late 30s when the Committee on Aviation Psychology, under Jack Jenkins, and later Morris Viteles, began providing psychological research services to the Civil Aeronautics Administration. This Committee paved the way for the now well-known Aviation Psychology programs in World War II and thereafter. Thus, men flew in airplanes for about 35 years before psychology was called upon for significant assistance. How

much different the role of psychology has been in man's early ventures into space!

After the first Russian and American satellites were sent into orbit in late 1957 and early 1958, the scientific resources of our nation were mobilized and organized to meet the challenges of the new space frontier. That psychology had an important role to play is apparent from its inclusion on several new scientific committees to advise on space flight problems. The National Academy of Sciences set up a new Space Science Board with S. S. Stevens representing psychology. A Committee on Man in Space has been established quite recently under the Space Science Board. On this Committee, psychology is represented by John W. Senders.

In late 1958 an Armed Forces NRC Committee on Bioastronautics was established to advise the Armed Forces and other agencies. Under this committee were panels representing ten different technical areas, one of which was psychology. I had the honor of serving as chairman of this Panel on Psychology. Other psychologists on the panel were Paul M. Fitts, William Bevan, John L. Brown, Abraham Carp, George T. Hauty, John W. Senders, Richard Trumbull, and Gordon A. Eckstrand. In addition, the panel included three psychiatrists, John C. Lilly, George E. Ruff, and David McK. Riock.

Because of the increasing role of the National Aeronautics and Space Administration in space activities, this panel, and the parent Committee on Bioastronautics, had a rather short life of about two years. During this time, however, the Panel on Psychology made some noteworthy contributions. Advisory recommendations from the panel were applied to Project Mercury for both animal and manned flights. In addition, the panel prepared and published reports on training of astronauts (Anonymous, 1961), sensory and perceptual problems related to space flight (Brown, 1961), and human performance as a function of the work-rest cycle (Ray, Martin & Alluisi, 1960).

Much more important than the part-time work of psychologists on national space flight advisory groups has been the work of several psychologists intimately involved in the Mercury Project of the National Aeronautics and Space Administration. Our profession can be justly proud of the outstanding work of Robert B. Voas, whose key role in the Mercury Project is fairly well known. Less

well known are the contributions of other psychologists, not in NASA, who have also made noteworthy contributions.

In a program as complex as that of Project Mercury, combining the efforts of many thousands of individuals, it is difficult to isolate the contributions of particular scientists. There are, however, quite a number of psychologists who have made important direct contributions to the Mercury Program and many more who contributed in ways which are less apparent, but, nevertheless, important. I will try to show some of the significant psychological contributions which I can identify.

No discussion of psychological contributions to Project Mercury would be complete without some mention of the selection program whereby the seven astronauts were chosen. Quite a large number of psychological tests were used in this selection program. Besides Robert Voas, the psychologists who played important roles in planning and carrying out the selection program were David Trites, of the Air Force Personnel Laboratory, and William O'Connor, of the Naval School of Aviation Medicine. Since the Mercury astronaut selection program has already been well publicized, I will not go into it further here, but go on to discuss some of the vehicle design, training, and human performance aspects.

In the early planning of Project Mercury, it was generally thought that the extreme environmental stresses and the compressed time scale at the high speeds would make the pilot quite useless as a controller. Thus, it was thought that all vehicle functions must be automatically controlled just as in an unmanned satellite. The role of the astronaut was considered to be that of a passive passenger or a biological specimen with little, if any, control of the vehicle entrusted to him.

As the Mercury Program advanced, the planned role of the astronaut became more and more like that of an aircraft pilot with some of the direct control of the vehicle in his hands. For many other functions that were automatically programmed and controlled, the astronaut was provided with emergency controls as back-ups to the automatic systems. Thus, the astronaut could contribute much to the potential success of the mission and to his own safety.

Before reviewing the specific contributions of psychology to Project Mercury, let us first look at

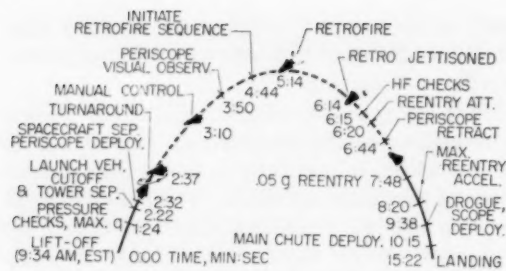


FIG. 1. Major events during Mercury MR-3 flight.
(From Shepard, 1961.)

some of the pertinent data (Shepard, 1961; Voas, Van Bockel, Zedekar, & Backer, 1961).

Figure 1 shows the major events in the first Mercury (MR-3) flight by Alan Shepard on 5 May 1961. Along the outside of the parabolic curve, representing the ballistic flight path, the major flight events are indicated. Under the curve is shown the time of the event in minutes after launch. Of particular interest to us here is that part of the flight from booster separation to the beginning of reentry. This constituted the weightless portion of the flight. But, more important, I think, is the fact that during most of this period Shepard had manual control of the attitude of his capsule.

A general indication of how Shepard's time was occupied during the flight is shown in Figure 2. Almost continuously he was engaged in communication with the ground as shown by the line near the bottom of the diagram. Of particular interest is the time during which he actually controlled attitude of the vehicle.

From the motion picture records obtained during the flight, Voas and his staff were able to plot the eye movements in terms of the area of the capsule being observed. A link diagram of eye-movement shifts during a weightless portion of the flight is shown in Figure 3. The time period covered Shepard's first attempts to control attitude. The diagram shows that the attitude indicator, the periscope, the clock, and the manual fuel handles were occupying most of his attention. This eye-movement link diagram is much like the eye-movement diagrams that Paul Fitts and his associates plotted from their studies of eye movements of aircraft pilots.

Probably the most interesting psychological data from Shepard's flight is that relating to his manual control of vehicle attitude. The control of attitude

was a rather difficult tracking task. Control was accomplished with pairs of small rocket jets for each rotational axis. Thus it constituted an acceleration tracking task, about three axes. This task had been thoroughly practiced by Shepard and the other astronauts on various training simulators. One of these was a so-called procedures trainer, resembling a fixed-base aircraft flight simulator. In Figure 4 we see separate records for attitude control in pitch, yaw, and roll, shown as heavy lines. These are shown against a shaded background made up of the area enveloped by records from ten preceding trainer runs. The comparison shows that performance in actual flight was equal to that on the trainer. Certainly there is no evidence that the performance was degraded by weightlessness.

In obtaining these data on astronaut performance as well as in guiding the training of the Mercury pilots, Voas played a dominant role as a member of the Project Mercury team. I would like to cover briefly the work of several other psychologists who worked closely with Project Mercury and made significant contributions to the program.

As one of the major subcontractors to the McDonnell Aircraft Corporation, the Minneapolis-Honeywell Regulator Company developed much of the complex control equipment in the Mercury vehicle. At the initiation of this development, there were important questions about how much control a human pilot could be expected to exercise under the stresses and speeds of space flight, and how to design the control system and related equipment to be used by the pilot. John Senders and his associates at Minneapolis-Honeywell made a thorough analysis of pilot's work load during Mercury flights, assuming different modes of opera-

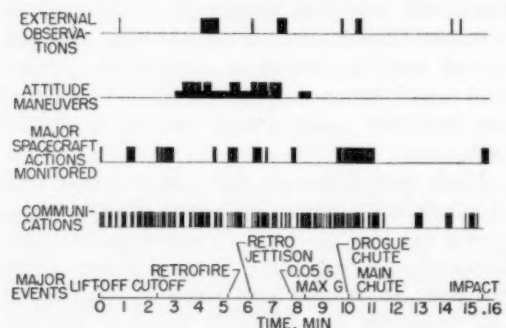


FIG. 2. Pilot activity schedule during Mercury MR-3 Flight.
(From Voas, Van Bockel, Zedekar, & Backer, 1961.)

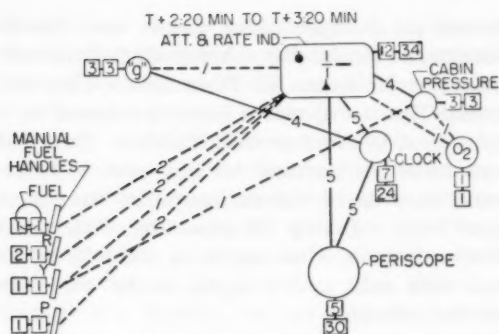


FIG. 3. Eye movement link diagram for weightless period of Mercury MR-3 flight. (From Voas, Van Bockel, Zedekar, & Backer, 1961.)

tion and possible flight emergencies. This analysis used several engineering psychology techniques, including task simulation using analog computers, human transfer function analysis, and work-load (or task) analysis. This analysis demonstrated that the probable work load would be manageable by the pilot and that the pilot could manually control attitude of the vehicle during most phases of the Mercury mission.

One of the more specific inputs to the Mercury vehicle by John Senders and the group at Minneapolis-Honeywell relates to the attitude indicator. The attitude indicator is quite different from conventional aircraft attitude indicators in that the pitch and bank indications are on separate dials. Also novel is the manner of showing rate information at the center of the instrument. It appears to me that this novel integration of attitude and attitude rate information is excellently suited to the Mercury vehicle and mission. Without wishing to detract from the accomplishment of the Minneapolis-Honeywell group, it is of interest that earlier versions of this instrument have been studied for possible aircraft use both at our laboratory and in England.

Other major contributions to the design of the Mercury vehicle were made by Edward Jones and his associates at the McDonnell Aircraft Corporation where the Mercury capsule was designed and built. To use Jones' own term, we can describe the work of his group as "man's integration into the Mercury capsule." In other words, they did the human engineering of the system. One of the more interesting activities of the McDonnell group was their Failure Task Analysis. This consisted

of an analysis of possible equipment failures that could occur during flight, a determination of the symptoms which the pilot or ground crew would have of the failure, the appropriate actions to take, and the consequences to be expected. It is easy to see the value of such a failure analysis for making improvements in the design, and for crew training. Much of the astronauts' training was based on this failure analysis. Although some type of failure analysis in design of complex systems is not new, I believe the McDonnell analysis was considerably more complete and detailed than any previously done, in terms of the cues to the pilot and the actions he could take. I fully expect that this technique will set a pattern for human engineering of future manned space vehicles.

Other important efforts of Jones and his group dealt with the visual problems of the astronaut, both inside and outside the Mercury capsule, and with the design of the procedures trainer (or simulator) to which I have already referred. In commenting on the training for his flight, Astronaut Shepard (1961) attached high value to two training devices, one of which was this procedures trainer.

The other trainer that Shepard felt was of great value was the Navy human centrifuge at Johnsville, Pennsylvania. On this centrifuge, operating through a computer, the astronauts were able to experience the accelerations of flight as well as effects of their own control of the capsule. In the work with the Johnsville centrifuge we again find psychologists playing an important role, in this case working very closely with Navy physiologists and medical personnel. John L. Brown (1960)

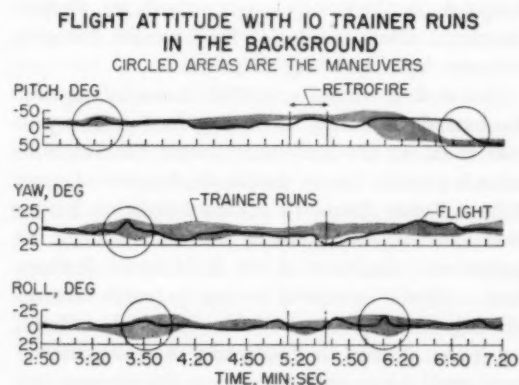


FIG. 4. Pilot control of attitude during Mercury MR-3 flight. (From Voas, Van Bockel, Zedekar, & Backer, 1961.)

and, more recently, Randall Chambers have obtained performance measures of X-15 pilots and Mercury astronauts on the Johnsville centrifuge. Their experiments demonstrated that with properly designed hand controls the pilots could maintain quite good tracking performance while exposed to high acceleration forces. Their experiments included the type of accelerations required to place a manned satellite into an earth orbit and bring it back into the atmosphere. These studies, plus the training which the astronauts obtained on the Johnsville centrifuge, contributed greatly to the Mercury Program.

Besides the problem of high accelerative forces during launch and reentry, there has been the long-standing question of how men will be affected by the weightlessness that becomes the normal state of affairs in space. For psychologists, the primary question has been, how will the pilot or astronaut's work performance be affected by lack of gravitational force? The only satisfactory way of producing weightlessness without actually going out into space has been by flying an aircraft in a parabolic path so that gravity is exactly counterbalanced by centrifugal force. The first significant experiments on human performance as affected by weightlessness were done by Gerathewohl, Strughold, and Stallings (1957) in fighter-type aircraft at the Air Force School of Aviation Medicine. Later, experiments were conducted by Brown and others at our laboratory with subjects tied down as well as free floating in a cargo-type airplane. These experiments (Loftus & Hammer, 1961) proved that human performance would not be disrupted or even significantly affected for subjects strapped in their seats, as planned for Project Mercury. Thus another of the potential obstacles to space flight had been dispelled.

Just as kings of old protected themselves against the hazards of eating by having food tasters precede them, so the modern astronaut likes to have animals precede him to sample the hazards of space flight. Before Shepard's Mercury flight on 5 May 1961, a chimpanzee, Ham, was put through an approximate duplicate of the flight on 31 January 1961. Ham's successful return to earth verified not only the physiological safety of such a flight, but it also verified that voluntary manual performance could be carried out during the stresses and strains of a rocket launching, weightlessness, and reentry. The credit for Ham's performance goes

to two psychologists, Fred Rohles and Marvin Grunzke, at the Air Force Aeromedical Field Laboratory at Holloman Air Force Base. They had trained Ham to operate a lever in response to a light signal to avoid an electric shock. Ham had been trained to continue his task even while exposed to some of the environmental stresses of space flight. During his pioneering flight in a Mercury capsule, Ham continued faithfully at his task with only a few lapses during the most stressful periods.

From the foregoing samples, we can see that psychology and psychologists have made important contributions, along with many other sciences and scientists, in the early phases of our nation's space efforts. These contributions have been generally in the selection of astronauts, the determination of astronaut capabilities and design of the work station to match these, and the design of training equipment and methods for astronaut training. These are the traditional areas of psychological contribution to personnel selection, human engineering, and training.

What has already been accomplished is only a small beginning by comparison with the work needed to meet the challenges of the future. Looking ahead a decade or two let us examine some of the activities we can reasonably expect human beings to perform, or attempt to perform, in space.

The following are some of the jobs now foreseen for future astronauts:

1. Work and live in a relatively small space vehicle for weeks or months.
2. Rendezvous and join up with another space vehicle in an earth orbit.
3. Move about the interior and exterior of a space vehicle and transfer from one vehicle to another.
4. Assemble or construct space platforms in an earth orbit.
5. Land a space vehicle on the moon, explore the surrounding surface, and return to earth.

All of these activities, of course, must be carried out under the handicap of an extremely hostile environment, including weightlessness (except on the moon), lack of atmosphere, extremes of temperature, and radiation. Even a superficial analysis tells us that here are many challenging problems for psychological research. Let us review a few of the problems and see how some psy-

chologists are already subjecting them to vigorous attack. To keep it within manageable proportions, I have chosen to restrict this review to work either carried out or sponsored by our Behavioral Sciences Laboratory at Wright-Patterson Air Force Base.

In discussing contributions to Project Mercury, I have already mentioned research studies on human performance under weightless conditions, as obtained for brief periods in aircraft. From these aircraft studies and the brief flights of Shepard and Grissom, we are assured that the performance of a man strapped in his seat will not be impaired significantly by weightlessness.

The situation is quite different, however, if the man must move about and do work inside or outside his vehicle. For him the lack of gravity is a great inconvenience. Fiction writers have long ago solved the weightless man's problems by giving him magnetic shoes with which to walk on the walls and ceilings and a personal rocket gun with which to propel himself from place to place. These ideas of the fiction writers have been tested in our zero-g airplane by Brown, Simons, Gardner, and others (Loftus & Hammer, 1961; Simons, 1959; Simons & Gardner, 1960). Unfortunately, the problems are not so easily overcome as thought by the fiction writers.

For purposes of walking, magnetic shoes, suction cups, and other means of gluing the feet to a surface, have so far proved to be unsatisfactory substitutes for gravity. They hold only when touching the surface and lose their hold or pull when separated from the walking surface. Whereas the magnetic shoes and other adhesion methods fail to replace gravity for purposes of walking, they seem to be a good gravity substitute for human perception of the vertical. For all of our subjects who have tried the magnetic shoes, the ceiling of the airplane has immediately become the floor, or downward direction. Evidently, for the standing man under weightless conditions, down is always the direction where his feet are attached.

The fiction writer's personal rocket gun, for the weightless astronaut to use in moving from place to place, also turns out to have many weaknesses. Unless this thrust can be directed precisely at the center of mass, the subject's body will be thrown into a spin instead of being propelled in the desired direction. Unfortunately, men are not endowed with a sense organ to tell them the location of the center of mass. To make the personal rocket

gun useful, therefore, will require some elaborations and improvements on the basic idea. Several of these are now being studied.

That men will ever propel themselves in open space with hand-held rocket guns seems somewhat doubtful. It is more likely that they will move from one vehicle to another within a small capsule with suitable rocket propulsion. Certainly, we can expect men to be guiding a space ship so as to rendezvous with another ship in approximately the same orbit. But the execution of such a rendezvous presents rather formidable problems in guidance and control. Any change in velocity of an orbiting object will cause it to seek a new orbit. Suppose a man in an orbiting capsule were to increase his speed to rendezvous with another satellite ahead of him in the same orbit. Because of the increased speed his capsule would move out to an orbit of greater radius, but with a longer time per orbit. Thus, his capsule would move above and then drop behind the target satellite. Similarly, if the man were to decrease the speed of his capsule it would drop to a lower but faster orbit. For executing a rendezvous, therefore, we truly have a case where the shortest distance is not a straight line, but an arc. This arc will end at the target satellite only if the amount and direction of thrust have been properly chosen.

Besides having complex control problems in executing a rendezvous with another space ship, an astronaut faces rather difficult problems of visual perception. Usually his visual target, the other ship, will be seen against a background of black sky, punctuated only by stars. Usually part of the other vehicle will be brightly illuminated by direct sunlight, part of it by earth reflection, and part of it will be completely dark. The earth may or may not be visible. Thus, orientation cues for direction may be lacking. Here is a fertile field for visual perceptual research, which Baker and Steedman, in our laboratory, have begun to explore in a specially prepared dark room. Their research has been directed toward obtaining thresholds for the kind of target movements which an astronaut would be required to judge in visually controlling a rendezvous in space. A sample of Steedman and Baker's (in press) results is shown in Figure 5. Thresholds were determined for target motion toward and away from the observer, using targets of different angular sizes. Note that for the largest target size the direction of movement can be judged

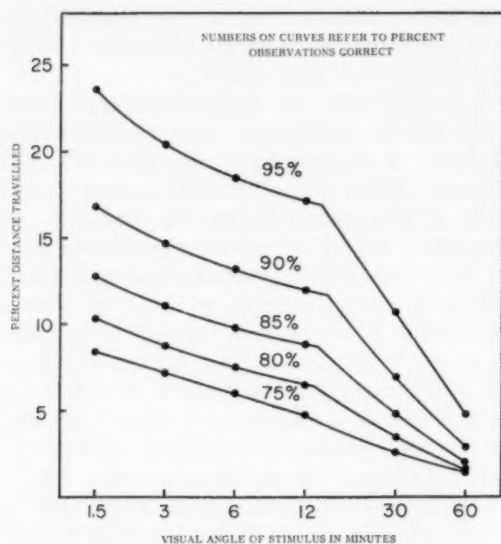


FIG. 5. Thresholds for judgment of target movement under simulated visual conditions of space. (From Steedman & Baker, in press.)

correctly (90 per cent threshold) in less than 5 per cent of the observer-to-target distance. The curves have a break at about 12 minutes of visual angle. At the smaller visual angles it appears that the target motion is judged not by size change, but by a change in brightness much as in Ricco's Law. At the larger visual angles the subject judges the target motion by the change in stimulus area.

A study of the human problems of performing assembly and construction operations in space has led several people in our laboratory to return to a traditional psychophysical technique that is almost as old as experimental psychology. This is the measurement of thresholds for lifted weights, but with a space age twist. In space, objects have no weight, but their mass remains unchanged. Thus, it is of interest to know how the thresholds for mass alone compare with the thresholds for both weight and mass. Furthermore, we expect that in space many operations will have to be performed by remote manipulators such as those used in nuclear laboratories. Figure 6 shows some sample results from the studies of Rees and Copeland (1960), and Crawford and Kama (in preparation), comparing normal thresholds for weight (and mass) with comparable thresholds for mass alone, handled directly and through a remote manipulator. The thresholds for mass alone were obtained on a

frictionless (air-bearing) table. Some measurements were also made under true weightless conditions in our zero-g airplane, which verified the validity of the results obtained on the frictionless table.

A final human problem of space flight which I will discuss is the problem of long-term work and confinement in a very small vehicle. Once placed in orbit, an earth satellite can keep going almost indefinitely without the application of additional thrust. If such a vehicle is manned, the limiting time that it can remain aloft is most likely to be the tolerance or endurance of the human crew. If the flight time exceeds about 24 hours, we must provide additional men for crew relief. Thus, we have the problems of how large a relief crew must be added and how the duty and rest periods should be scheduled. The situation is somewhat similar to that on a submarine that may remain submerged for weeks or months, except for, at least, one important difference. Unlike the submarine, the space ship has a severe weight problem. To place each pound in orbit requires several hundred pounds of rocket thrust. Thus, to save weight, it is of the utmost importance that the total number of crew members be an absolute minimum; and these crew members must live and work in a very confined space.

Research on the problems of long-term confinement and work-rest cycles has been conducted for our laboratory by Adams and his colleagues at the Lockheed Aircraft Corporation at Marietta, Georgia. Chiles has monitored this program for our laboratory. The Lockheed research has been conducted in a crew station mockup with duty stations for up to five crew members and a small additional area for sleeping and eating. At each

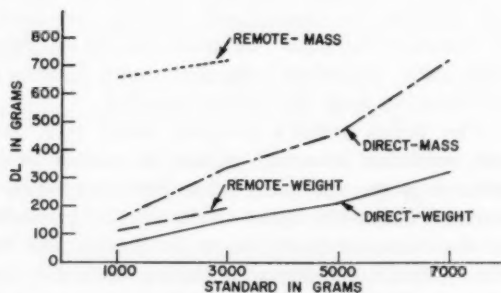


FIG. 6. Thresholds for judgment of weight and mass. (From Crawford & Kama, in preparation; Rees & Copeland, 1960.)

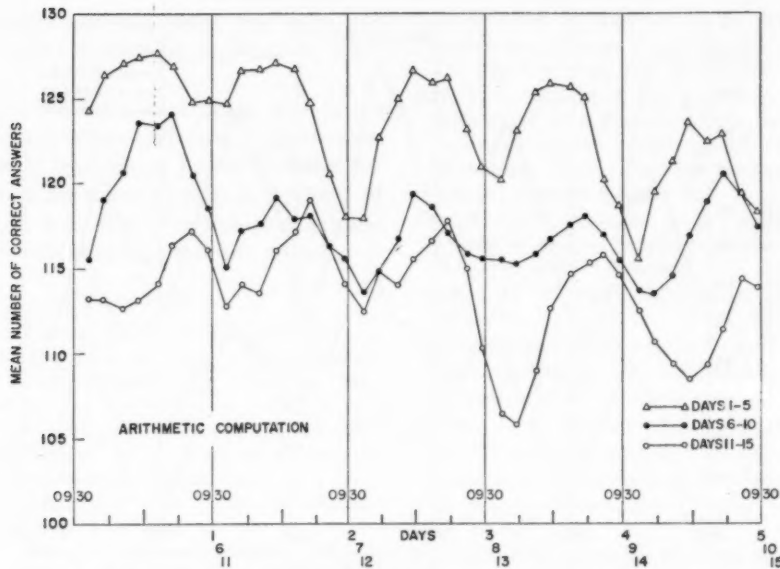


FIG. 7. Performance on arithmetic computation task during 15 day confinement.
(Adams & Chiles, in preparation.)

duty station is a work panel providing the subjects with the following tasks: probability monitoring, auditory vigilance, reaction time to warnings, and arithmetic computation. On all of these tasks performance is continually recorded and there is provision for some recording of physiological measures.

A major experiment (Adams & Chiles, in preparation) conducted in the Lockheed mockup used two B-52 crews supplied by the Strategic Air Command. Each crew spent 15 days in the mockup on a work-rest cycle of 4 hours work and 2 hours rest for the entire 15 days. In Figure 7 we see the performance curves for arithmetic computation,

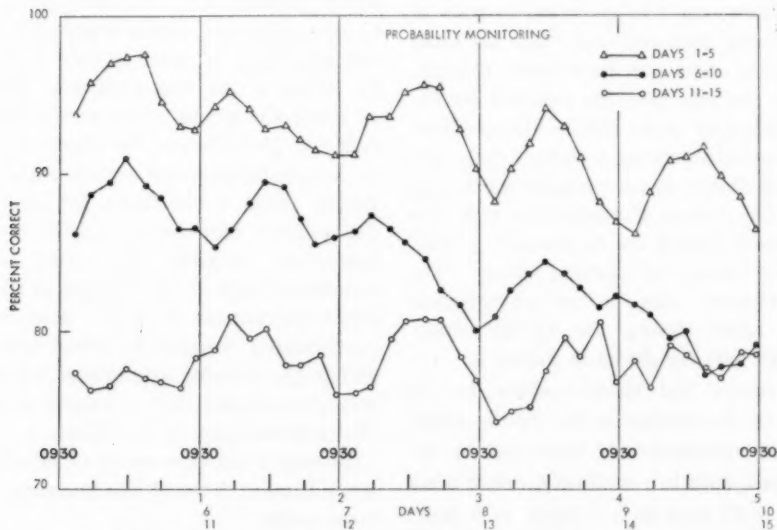


FIG. 8. Performance on probability monitoring task during 15 day confinement.
(Adams & Chiles, in preparation.)

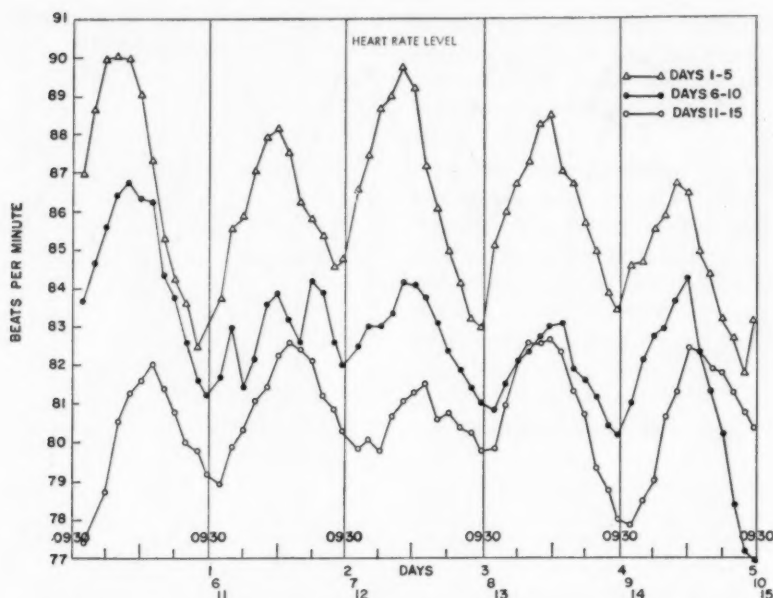


FIG. 9. Variations in heart rate during 15 day confinement. (Adams & Chiles, in preparation.)

and two results are particularly worth noting. Through the 15-day period there is only a moderate decline in performance in spite of the rather severe work-rest cycle of 4 hours work and 2 hours rest. The other significant finding is the diurnal cycle in the performance level, with the low point during the morning of each day. This cycle persisted throughout the 15-day period in spite of the subjects having been removed from the usual day-night routine and environmental changes. Figure 8 shows the same findings repeated for the probability monitoring task. All of the performance tests showed evidence of a diurnal cycle and only a moderate decline in performance during the 15 days. On the pattern discrimination task performance improved during the confinement period, almost certainly because of learning effects. The daily cycle appeared also in the physiological measurements taken during the 15-day study. Results for heart rate are shown in Figure 9.

From the results just shown, added to the verbal reports of the subjects in the 15-day study, we believe that well-motivated men can be expected to live and work in a confined space or other vehicle for up to 15 days on a schedule of 4 hours work and 2 hours rest. This means, of course, that the number of crew members over and above

the duty stations to be manned need be only 50 per cent. Or, put another way, only three men could operate two duty stations on a continuous basis provided the three men are cross-trained on both duty stations.

There is, of course, much additional work by which psychologists are contributing to our nation's current and future space efforts that I have not attempted to cover in this review. There is, for example, the important and interesting work of Brant Clark and others at the Navy School of Aviation Medicine on the effects of slow rotation on human performance. Such rotation of a space vehicle offers a substitute for gravity in space. There have been many studies involving water immersion, isolation, and other techniques for simulating some of the changes in the sensory and social environment of space. Also, there are many psychologists working in government laboratories and in the guarded interiors of our major aircraft and other corporations, helping to shape the designs of our space ships of the future.

In concluding this review of psychology and the space frontier, I would like to restate several points made earlier:

1. Psychology as a science has a vital role to play in the conquest of space.

2. Psychologists have responded with vigor and imagination to the challenging problems of space flight.

3. The space flight challenge brings with it the stimulation of new ideas and opens opportunities for new types of research on human behavior.

4. Psychology has much to gain in the form of new knowledge, and, also, in status as a science, from its participation in the push into space.

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CONTRASTING SYSTEMS OF WORK ORGANIZATION¹

RAYMOND A. KATZELL

New York University

WITHIN the professional lifetimes of most of us, industrial psychology has been undergoing a profound change in the strategy of its utilization in industry and government. It was not very long ago that our function was essentially limited to serving as a kind of technical adjunct to the Personnel Department: polishing and patching up various personnel techniques like the employment interview or the merit rating plan. Increasingly, however, we are being asked to help design and redesign the very fabric of the industrial organization: that system of goal-directed relationships by which men and managers work. Management, labor, and society at large seem to have some hope that we can contribute at the policy level to the design of productive systems and enterprises in a way that will further both their economic and psychological missions.

But, in meeting this challenge, what is the master plan or blueprint that we should use in designing these organizations and work systems? What theory of human behavior in organizational settings should guide our research and counsel? This is a question to which industrial psychologists have begun giving increasing attention, and one which will require much more thought and research in the years ahead. It is the question which lies at the heart of these remarks.

As you well know, students of organizational concepts have typically classified the extant master plans or theories into two broad categories. One of these categories has been variously labeled as traditional, classical, mechanistic, bureaucratic, autocratic, or "Theory X." The other has been called modern, human relations, democratic, participative, or "Theory Y."

The first of these categories owes its intellectual debt on the one hand to the early scientific management and administrative theory people like F. W. Taylor and Henri Fayol, and on the other hand to the sociological theory of bureaucracy put forth

by Max Weber. The second general school of thought is largely the product of behavioral scientists, having its origins particularly in the work of Elton Mayo and Kurt Lewin, and colored to some extent by the thinking of personality theorists like Freud, Carl Rogers, and Kurt Goldstein.

From what I've already said, I know that you have a pretty good idea of the ingredients of each of these two blueprints or conceptions of productive organizations. However, let me pin it down a bit further, just to be sure of our definitions. To do so, I will cite an apt summary of these two general approaches which was written by Shepard a few years ago. He contrasted the "new" approach with the "traditional theory" in the five following respects:

1. Wide Participation in Decision-making Rather than Centralized Decision-making. . . .
2. The Face-to-Face Group Rather than the Individual as the Basic Unit of Organization. . . .
3. Mutual Confidence Rather than Authority as the Integrative Force in Organization. . . .
4. The Supervisor as the Agent for Maintaining Intragroup and Intergroup Communication Rather than the Agent of Higher Authority. . . .
5. Growth of Members of the Organization to Greater Responsibility Rather than External Control of the Members' Performance of Their Tasks . . . (1956, pp. 262 f).

We all recognize, of course, that this is intended merely as a caricature of the two points of view, that it doesn't cover many of the details of their exposition, that within each category there are some differences among the various proponents of the general point of view, and that there are really subclassifications of each; for instance, Likert (1961) has recently subdivided the two broad categories into four.

However, I have neither the time nor the desire to undertake the detailed dissection of these conceptual positions at this time. By painting these viewpoints in broad strokes, I trust that I have served my immediate purpose of introducing the problem of organizational design and the general kinds of thinking that have been going on about it. The main question of present concern is to what

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extent we can allow ourselves to be guided by these theoretical positions in our future efforts at organizational design. In other words, have the results obtained by the adoption of one or another system of organizing human effort been such that we can employ it as the master plan to guide ourselves and our clients?

My own conclusion from the evidence at hand is that we have at present no one conception or strategy of work organization that is unequivocally or universally superior to others, in terms of results achieved.

The reason for this conclusion goes beyond the scarcity of good evaluative research, although there is no doubt that we are handicapped by this condition, too. Mainly, I have reached this conclusion because the research to date has failed to support consistently the virtues or advantages of any given organizational scheme.

The malfunctioning of the classical, directive, or bureaucratic systems of organization has been so thoroughly aired in recent behavioral science literature that it scarcely requires further documentation before this audience. Let me simply remind you of some of the major effects which have been identified, or at least alleged: restriction of output, conflict among organizational subunits, rigidity of job performance resulting in poorer service to clients, low productivity, low job satisfaction, resistance to and withholding of innovations and improvements, hostility to superiors, feelings of apathy and failure, and arrested personal development.

The alternative strategy of organizational design, of the democratic, participative, human relations type, has of course been proposed as a corrective to these unsavory consequences; and, as you well know, there is a considerable body of research which supports this hope.

Unfortunately, however, the story isn't that simple, for there are also a number of studies that raise questions concerning the general superiority of the newer conception. Let me mention just a few of these, by way of illustration:

1. Several studies have indicated that, while participative patterns may be associated with higher levels of job satisfaction than directive patterns, the latter are no less productive and may be even more productive. These kinds of results have been found in research units by Weschler, Kahane, and Tannenbaum (1952), in an office situation by

Morse and Reimer (1956), and under laboratory conditions by Leavitt (1951).

2. Moreover, evidence is accumulating that employees' reactions to participation are not uniformly favorable. Recent research in a Norwegian factory showed that the results obtained varied with such factors as the perceived legitimacy and relevance of participation (French, Israel, & Aas, 1960).

3. A case study by Argyris (1959) has indicated that in a plant operating under a rather conventional and old-fashioned organizational philosophy, the workers were quite productive in management's view; they were on the whole well satisfied with their jobs; turnover was low; a change in production technology which was made without worker participation generated little opposition or disruption; and there were other evidences of a smoothly operating, productive, and satisfied work force. Perhaps most interesting was the fact that most workers had high self-actualization scores, i.e., indicated that their main motivations and needs were being adequately met through their work.

4. The "Man on the Assembly Line" study indicated that those workers generally had little intrinsic job satisfaction, worked in social isolation rather than as members of closely knit teams or groups, and had little opportunity to participate in planning their work or setting organizational goals and objectives (Walker & Guest, 1952). Yet the production efficiency of this kind of organizational system is widely conceded, and its ability to attract and hold competent personnel is also clear.

5. Several studies have shown that more democratic supervision is by no means universally preferred to more autocratic supervision. For example, research at General Electric revealed that factory worker job satisfaction was higher under either strongly autocratic or strongly democratic supervisors than under supervisors who were intermediate on the scale (Public and Employee Relations Research Service, 1958). Also Foa's (1957) study of Israeli sailors showed that those crews which had expectations of authoritarian supervision were as well satisfied with a commander having that kind of attitude as with one who was permissive.

The foregoing are just a few of the facts that can be assembled to support my point that no single pattern or system of work organization and manage-

ment has yet been demonstrated to be unequivocally and universally superior to another or others.

Perhaps one reason for this is that these systems have each tended to represent a blind man's view of the elephant. As Bennis (1959) has put it, classical organization theory has addressed itself to "organizations without people," and human relations theory has tended to think in terms of "people without organizations." The implementation of either of these fragmentary conceptions therefore can be expected to breed its own kind of disaster, and indeed we have ample proof that it has.

Does this mean that a new blueprint—let us call it "Theory Alpha and Omega"—which somehow combines the best of the other two patterns, is the answer? Well, if it takes the general form of these other theories, I am still pessimistic.

To explain this comment, it is necessary to backtrack a bit and review the *form* of these theories, in contrast to their content or substance. Theories cast in the form of classical organization theory or human relations theory are, when you stop to think about it, rather peculiar anomalies in the field of science. They are a strange admixture of empiricism and ideology, of facts and prescriptions, of principles and proverbs. Whereas scientific theory is positive or descriptive in formulation, organization theory has tended to be normative or prescriptive; whereas scientific theory is largely developed by induction from facts, organization theory has relied heavily on deduction from assumption; and whereas scientific theory is proposed as valid for a specified universe, organization theory is often promulgated as general truth.

Of course, descriptive scientific theory can also be legitimately employed for prescriptive or normative purposes. The formulation for such utilization is: if you want such-and-such results, under so-and-so circumstances, research indicates that you should do thus and so. If you want to relieve periodic headaches, wear eyeglasses when reading, provided that your eyes are defective and you are not suffering from a brain tumor. Similarly, if you want your employees to be satisfied with their supervisor, then research shows that you should get the supervisor to use democratic methods, provided that the employees have expectations of such supervision, that the supervisor has upward influence, and that this supervisory method will not have consequences prejudicial to certain other of your objectives.

But organization theories, of the type we have been discussing, do not commonly follow this formulation. Rather, by concerning themselves with certain preselected dependent variables (e.g., efficiency or worker satisfaction) and not with others that are relevant to the system, by relying heavily on unproven assumptions, and by their tendency to generalize rather than paying due attention to surrounding circumstances, such theories typically take the form of a blueprint or master plan which is prescribed as *the* one best way to organize and manage all work. Hence my belief that no new master plan, similarly arrived at, is the answer to our need for guidance in consultation and research on organizational design.

Instead, what is needed is a scientific, descriptive theory of organization which spells out the relationships among given dependent variables (like man-hour productivity or satisfaction with supervision) and various independent variables (typically organizational characteristics or practices), under various situational conditions or parameters (such as the personalities of the participants or the production technology). The outcome of such an organizational theory would not be a blueprint or approved pattern, but rather a defined network of relationships among phenomena that permits one to adopt a course of action which, in the light of the circumstances defining the situation, is most likely to achieve some approximation to optimum results among the valued criteria or dependent variables. Incidentally, I would like to cite the books by March and Simon (1958) and by Stogdill (1959) as illustrating this *approach* to organizational theory building, irrespective of the particular content of their formulations.

Of course, we have no such comprehensive theory of organization ready for use. Even the aforementioned books are admittedly incomplete and contain numerous untested propositions. But we do have some subtheory, covering a segment of the total picture, which, if not exactly mature, is at least beyond its infancy. I am referring particularly to theory in the domain of leadership and supervision, as illustrated by the recent works by Bass (1960) and by Likert (1961). Such theory can certainly serve as useful guideposts to consultation and further research in this area.

Needless to say, a lot more research is also needed in the total field of organization theory.

I would like to touch briefly on two sorely needed research areas, not so much in terms of specific content, but more in terms of research strategy.

The first of these areas has to do with the variables and dimensions describing organizations and work systems. I believe you will agree, on a moment's reflection, that the "state of the art" is particularly primitive in this respect. For example, March and Simon (1958) provide a list of the variables they incorporated in their theoretical treatment of organizations. There are 206 of them! Some of them are relatively straightforward and operational, like "age of participant" or even "satisfaction with job." But others are complex concepts, that are really hypothetical constructs rather than variables, such as "motivation to reduce conflict" and "munificence of the environment."

But beyond the problem of measuring such constructs, the sheer number of variables that can be used to describe organizations is staggering. If we are to work with them meaningfully, we need somehow to reduce them to a smaller number. In other words, we need to discover what genotypic dimensions underlie the phenotypic complexity of organizations. For example, there are many specific practices, activities, and variables that take place in organizations and which determine upward influence: who talks to whom and how often; the frequency, composition, and agenda of group meetings; the distribution of information and knowledge; whether or not there is a union; and so on. But shouldn't we be looking for ways to condense these diverse phenomena into an upward influence score, assuming that this is indeed a relevant dimension of organizational life?

In other words, if we take both of these points together—the need to develop measures of organizational phenomena and the need to discover their underlying dimensions—what I am suggesting is a program for organizations that is analogous to the familiar one psychologists have already initiated in connection with the description and measurement of individual personality (e.g., Cattell, 1946; Eysenck, 1953).

The second domain of needed research that I wish to mention at this time has to do with identifying the situation that conditions whether organizational practice A or B will yield better results. You will recall that our formulation of how descriptive theory can be put to use depends

on knowing how particular independent variables relate to particular dependent variables under given circumstances. It has further been my thesis that the surrounding circumstances or parameters play so large a part in determining the relationships between independent and dependent variables in organizations, that we can expect no one system of organization and management to serve categorically as our blueprint. It therefore follows that we need to address a lot of attention to the part played by the situation, both in our research and in our consultation.

There is already a growing acceptance of this idea in the area of organizational theory concerned with supervision, although even here there has not yet been enough systematic attention. Furthermore, our practices and research in supervisory selection and training are still lagging behind theory and research on the situational conditioners of what is effective supervisory behavior.

But if the situational influences on effective supervision are yet to be adequately explored, their effects on other kinds of organizational policies and practices have received hardly more than a glance. I refer, for example, to the situational influences on information systems, on compensation methods, on control systems, on organization structure, and on decision processes, to mention just a few.

What aspects or parameters of the situation should be looked into? Let me list a few which research and theory suggest are likely to be particularly influential determiners of what kinds of organizational policies and practices will work best:

1. The first one that I wish to mention is *size*, defined in terms of the number of interdependent members in the group or organization. To take an example, there are both theoretical and empirical reasons for believing that size makes a difference in patterns of effective supervisory behavior. Some years ago, Hemphill (1950) reported a laboratory study indicating that members were more favorable to direction and control by the leader in larger than in smaller groups; at about the same time, Worthy (1950) observed that supervisory behavior was characteristically more directive in large stores than in small ones. Size may also affect the distribution of roles and responsibilities; thus there is evidence that, as organizations grow, they involve a larger proportion of personnel in liaison and

staff functions (Haire, 1959; Terrien & Mills, 1955). In still another area, there is March and Simon's (1958) derivation to the effect that incentive payment schemes will work better in small than in large groups, with some empirical evidence to support this proposition (Marriott, 1949; Campbell, 1952). To be sure, as discussed in Indik's (1961) paper, the effects of size may be mediated by other variables, but there is ample evidence that size and its associated variables are likely to have marked influence on work systems and organizational patterns.

2. *Degree of interaction and interdependence* of organization members is another set of situational variables that may be important in several ways. Vroom and Mann (1960) have proposed that differences in this type of parameter may account for why autocratic supervisors were preferred in some groups and democratic supervisors in other groups within the same company. The relevance of interdependence for compensation systems is suggested by Deutsch's (1949) laboratory study: as compared to an individual incentive, a group incentive induced greater productivity in group activities which members performed interdependently, but not in those activities they performed as individuals. In the area of control systems, a case study of the Forest Service (Kaufman, 1960), where the rangers experience little interaction or task interdependence, indicates that control devices such as a carefully specified set of written rules, plus frequent inspections and other kinds of detailed reviews, apparently serve to channel rangers' behavior in directions aligned with the organization's mission; it is not unlikely that such control practices would backfire loudly if used with other professionals whose interaction and interdependence were much closer, say in a psychological consulting firm.

3. *Personalities of organization members*, including their motivations and expectations, constitute another type of conditioning variable that needs attention. I have already cited illustrations of the fact that such personality characteristics of subordinates affect the patterns of supervisory behavior to which they react best. Some research recently published by myself, Barrett, and Parker (1961) suggests that personality characteristics associated with small town vs urban culture also are important influences in employees' satisfaction with and performance in their total work setting.

4. *The degree of congruence or disparity between the goals of the organization and that of its employees* is a fourth factor that we may posit as affecting the consequences of various policies and practices. In general, we might expect participative methods of management to work well primarily when there is a reasonably high degree of congruence. Although this proposition receives much support from theory, and some from laboratory research, there has been little light thrown on it by research in ongoing organizations. However, this factor may be one reason why the Scanlon plan has particularly flourished in companies encountering tough sledding (Shultz, 1958); under these circumstances, the objectives of management and labor may be welded closer together in their joint concern for the company's survival. The notion also receives support from some of the organizational theorists. Likert, for example, writes: "The ability of a superior to behave in a supportive manner is circumscribed by the degree of compatibility between the objectives of the organization and the needs of the individuals comprising it" (1961, p. 115).

I am not at the moment raising the question of the optimum degree of integration between the organizational and individual goals. Shartle (1961), for example, has raised the interesting question as to whether high integration may not be a stagnating influence. But I do maintain that we need to recognize that in real organizations the degree of congruence ranges from much to little, and that its level in any given situation is likely to have an important effect on what management principles will or will not work well.

5. My fifth moderating parameter has to do with *who in the organization has the necessary ability and motivation to take action that will further its objectives*. Depending on the answer to this question, the organizational system that will work best is likely to vary markedly from one situation to another. Several theorists have recently arrived at propositions, which although more limited, are seemingly in accord with the point I am raising. Thus, March and Simon (1958) propose that where the task is simple in relation to the ability of the individual performing it, close supervision will be detrimental; but where the task is highly complex relative to the ability of the individual, then close supervision would function better. Working in a framework that is more motivational than cognitive,

Bennis (1959) has made the proposition that directive supervision, of the kind envisioned in traditional organization theory, would probably operate well in what he terms a "habit organization," that is to say, one whose major function is replicating standard and uniform products. On the other hand, he suggests that the management-by-objectives, consultative approach should work better in "problem-solving" organizations, concerned mainly with creating new ideas. This notion receives some support from the work of Baumgartel (1956), who reported that more effective scientific research departments are not much differentiated from less effective ones by the autocratic-democratic aspects of their supervisors, but the more successful ones are more likely to have a supervisor who is himself a dedicated and competent scientist who can set an example and provide technical help.

In the same general connection, we may cite the case study of coal mining by Trist and Bamforth (1951). In this study, a change in mining technology made the miners more interdependent, more of a team operation. Interestingly, the miners became increasingly hostile, often refusing to cooperate or to work. There were also more difficulties and hindrances in getting the work done. Thomas (1957), in his theoretical analysis of interdependence, raises a point that may account for such a result. He reasons that if the task is difficult for an interdependent team to perform, the members may hinder one another more than they facilitate each other's performance. This would produce tension, as well as poor production.

We therefore arrive at the proposition that the best way to organize and manage work is heavily conditioned by those in the organization who have the knowledge and motivation to get the job done. More specifically, it would seem necessary to mold the organizational system so that it maximizes freedom of action and initiative for those who can and will take effective action, while eliciting compliance, support, or noninterference from the others. Depending on the organizational loci of expertise and dedication, the appropriate work system may vary widely from situation to situation.

Thus, if my reasoning from theory and scanty data is correct, we need to examine the differential effectiveness of various organizational principles and systems in the light of situational parameters

or modifiers like the five I have just outlined: organizational size, degrees of interaction and interdependence among members, personality factors of organization members, the congruity or incongruity of goals of the organization and its members, and the loci of expertise and commitment in the organization. This list is, of course, nowhere near complete, but does contain some of what I regard as the more potent conditioning variables. In researching on their effects, we will naturally need to consider them not only singly but also in terms of their combined properties.

Let me conclude by summing up what I believe to be the substance and implications of my remarks:

1. I am doubtful about any comprehensive, normative theory of organization and work that says, in essence, "*This* is the way it should be done."

2. I believe, rather, that contemporary organizational theory would be more valuable if it were slanted more in a positive, descriptive direction. Quite possibly, we should be devoting relatively less effort to broad theories that aspire to a *Weltanschauung* of organizational life, and thereby run too far ahead of available data. Maybe it would be better to address proportionately more effort to limited, tighter theories concerning more circumscribed aspects of work systems, like supervision or controls or information networks. However, this is not to gainsay the desirability of developing such theories within a broad frame of reference.

3. I have reiterated the psychologists' battlecry: "More research is needed!" This slogan may be timeworn, but it has not yet outlived its relevance—especially in this field.

4. I have pointed to two domains in which more research is particularly needed: (a) on the identification and measurement of the cogent dimensions of productive organizations, and (b) on the study of the consequences of organizational methods and systems in the light of various situational parameters.

5. I have alluded to the fact that these problems have implications not only for theory and research, but also for practice and consultation. One of these implications is negative: beware of panaceas in patterning organizations and the relations among members. More positively, pending more guidance

from research on the impact of situational variables on the appropriateness of various organizational systems, let us at least be guided by theory and general wisdom in taking their possible role into account when advising with clients on their organizational problems.

6. In this interdisciplinary field of organizational theory, systems, and research, I am convinced that industrial psychologists can, because of our skills and opportunities, fruitfully take a more prominent part than we have. I hope that my remarks here may help to make this potentiality a reality.

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IMAGE OF INDUSTRIAL PSYCHOLOGY AMONG CORPORATE EXECUTIVES

M. R. FEINBERG AND J. LEFKOWITZ

City College of New York and BFS Psychological Associates

INTRODUCTION

IN spite of rapid recent growth, little research data are available on the image of industrial psychology among corporate executives. Stagner (1946) and Tiffin & Prevratil (1956) are two notable exceptions.

The present study concentrated on a sample of executive opinion in the following three areas:

1. Detailing the specific areas of subspecialization within the field of industrial psychology rated most favorably, as well as a general rating of the overall field.
2. Determining the differences in rating the value of industrial psychologists between executives possessing the authority to arrange for such services and executives without it.
3. Analyzing on the qualitative level the reasons for seeking the assistance of industrial psychologists.

SAMPLE

The sample consisted of 89 executives attending an executive seminar under the auspices of the American Management Association in the spring of 1960. The participants were representative of companies both large and small from different areas throughout the United States and Canada. Distribution according to company size is summarized in Table I.

METHOD AND STATISTICAL ANALYSIS

The questionnaire consisted of 13 items of the multiple choice, check list, and completion variety. A trial run of the questionnaire was accomplished on a smaller pilot sample of 25 executives approximately a year prior to the present study.¹ A sample question follows:

(1) Assuming you had the authority, would you hire an industrial psychologist?

Yes ——— No ———

¹ The authors are indebted to Mr. William Shepard, Program Director of the American Management Association, for assistance in the distribution of the questionnaire.

If so, check one or more of the areas in which, in your opinion, the services would be useful.

- a. Consumer Research
- b. Production Efficiency; Accident Prevention
- c. Employee Selection, Training, Placement & Promotion
- d. Executive Selection, Training, Placement & Promotion
- e. Employee Motivation (attitudes, morale, etc.)
- f. Human Engineering (redesign of machinery to match human thresholds)

RESULTS

A. Ratings of the overall field and of the various areas of subspecialization.

1. In response to the question whether they would hire an industrial psychologist 59 (66%) answered in the affirmative and 30 (34%) answered no.

2. Those who responded favorably to the question of whether they would hire an industrial psychologist were then asked simply to check the areas in which they thought the service listed would be especially helpful. Out of a total of 59 respondents the results, on a percentage basis, were as follows:

Employee motivation, morale	61%
Employee selection, training, promotion	54%
Executive selection, training, promotion	51%
Human engineering	27%
Consumer research	10%
Production efficiency, accidents	3%

TABLE 1

Number of employees	No. of firms
Under 500	16
500-999	8
1,000-4,999	25
5,000-9,999	16
10,000 and over	19
Information omitted	5
Total	89

TABLE 2
ATTITUDES TOWARD HIRING AN INDUSTRIAL PSYCHOLOGIST

Would you employ an industrial psychologist?	With authority	Without authority	Total
Yes	8	19	27
No	3	51	54
	11	70	81*

These values are significant at the .05 level using one-tailed test, significant at .01 level using two-tailed test (Siegel, 1956).

* The total is less than 89 because some questionnaires omitted pertinent information and thus could not be classified.

B. Association between rating and authority to hire the services of an industrial psychologist:

1. There is a significant relationship between whether or not an individual has the authority to hire and how he rated the field. Apparently executives with authority were more favorably disposed to hire a psychologist than those without it.

C. Reasons for seeking the service of an industrial psychologist.

A large majority of those executives who hired an industrial psychologist at some time did so because the problems that had developed were essentially psychological or "human" in nature. Psychologists were asked to effect a better understanding of employees, to cope with the problem of a "drinker" and to improve employee relations. The executives in our sample never hired a psychologist to deal with broad categories such as research or organizational structure.

Resistances, when a psychologist was considered but deemed inappropriate, revolved around such statements as "they are still in the process of justifying their worth," or "the expenditure outweighed the possible value."

DISCUSSION

It is recognized by the authors that they have no right to assume that the present sample is truly representative of the United States executive population. Participants in an executive development program may be atypical. However, in spite of this limitation there are still important implications and guidelines for future research emerging from the present investigation.

Industrial psychology, to fulfill its potential, should take a page from its own methodology and face the market—its criticism and overall evaluation.

This study has pointed out areas where executive acceptance is strongest and others in which a good deal of "image improvement" can be sought.

Most managers in the sample still think of the industrial psychologist in the traditional areas of morale, selection, and training. It would seem that education is needed to help industry view more favorably the role of an industrial psychologist as a general problem solver in market studies, advertising, and broadly gauged, more imaginatively conceived industrial research.

On the positive side, there appears a growing acceptance of the field. Stagner (1946) and Tiffin & Prevratil (1956) report 53% and 54%, respectively, as the percentage of executives who considered the employment of industrial psychologists as desirable. The present investigation reports a 66% favorability rating. This figure is especially encouraging since the present study surveyed executives from many different organizations while the two earlier studies were limited to either a single company or a specialized industry. The present questionnaire was not an exact duplicate of the ones used in these earlier studies. However, the general areas covered are similar, thus permitting these comparisons.

In addition, the investigators note a moderate acceptance on the part of corporate executives of human engineering, an area of subspecialization unrecognized within the field of industrial psychology a few years ago. However, market studies, in spite of all the controversies raging around motivation research, are still considered outside the province of industrial psychology.

Another finding worthy of note is the higher the management level the more favorable the rating of industrial psychology. Apparently the field seems to have gained greater acceptance among decision makers. If such a relationship is substantiated by further research, it might be desirable for psychologists to attempt broader education at top management levels in areas such as theoretical research and the role of the psychologist as a general problem solver rather than a staff specialist with limited professional skills.

SUMMARY

1. Almost twice as many executives stated that they would hire an industrial psychologist as compared with those who would decide against such a decision.

2. There are many executives still unconvinced that industrial psychology has any real worth at its present stage of development.

3. The areas of competence in which the industrial psychologist was judged to be most useful were morale, selection and training. Moderate acceptance was noted in the field of human engineering. Their services were deemed least useful in the fields of market research, production efficiency and accident control.

4. Those executives who have the authority to arrange for the employment of industrial psychologists within their organization tend to rate the field more favorably than executives without it.

Based upon an analysis of these data, generalizations and suggestions for future research were

offered. Most important, the field seems to have made advances since earlier studies. However, psychologists in industry are still viewed as having specific competence in the fields of morale, selection, and training. They are not called upon to initiate broad organizational research projects. Production efficiency and market studies are considered outside the general area of an industrial psychologist's competence.

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Comment

Insurance Coverage: Reply

In reply to the letter in the October 1961 issue of the *American Psychologist* from the members of the APA in the State of Michigan headed, "Insurance Policy: General or Specific," it appears to me that some implications are made about my activities as the author of the article in the December 1960 issue of the *American Psychologist* and as chairman of the BPA's ad hoc committee to investigate the problem of insurance coverage for psychological services. I feel that I should make clear exactly what occurred.

When the Board of Professional Affairs met on May 10, 1960, I reported as chairman of the ad hoc committee that I had been unable to arrange a meeting. Actually, I had set up a joint meeting with the Health Insurance Council (an association of companies selling health insurance), but this had to be cancelled because of a reorganization within that group. In any case, the Board was well aware of the fact that the committee had never met nor addressed itself to the problem.

However, the Board did proceed to a discussion of the problem. I participated as a Board member, but it was clear to all concerned that I was stating my own opinions and not those of the committee. The Board then took its action and asked me to write an article as a member of the Board (not as chairman of the ad hoc committee), giving an account of the general background of the problem and stating the Board's position. All of this should be clear to anyone who reads the article.

S. RAINS WALLACE
Hartford, Connecticut

Submissions to APA Publications

Two recent attempts to publish methodologically-oriented articles in APA journals, separated by over two years, and differing considerably in content but otherwise virtually identical, have pointed up some aspects of APA publication policies (or lack of same) which appear to be highly inconsistent with the purposes of a scientific organization.

The sequence of events in the case of both articles can be described as follows (only the particular journals involved varying in a counterbalanced order). (1) Submission of manuscript to first journal; (2) Return of manuscript immediately by journal editor, accompanied by letter containing highly favorable comments about content of paper, but concluding that it is not the type of material suited for that journal; (3)

Submission of manuscript to second APA journal, with covering letter reporting in full the action of the first APA editor; (4) Rejection of article by second journal editor, partially on the basis of comments by one or more consultants, most of which are irrelevant to the intended subject matter of the article, or reflect a negative bias in opposition to the author's position as expressed in the article (substantiated in at least one case by the subsequent discovery of the identity of the consultant), as distinguished from criticisms specific to the content of the manuscript itself. In the case of the second article, steps 3 and 4 were subsequently repeated with a third APA journal.

Each of these articles had previously been read by a number of psychologists, including in each case at least three consulting editors of APA journals, many of whom were kind enough to offer comments and criticisms. In fact, the decision to submit them for publication was based in part on the encouragement received from those who had read them. Since at least the first editor in each case apparently agreed that they were worth publishing, although in some other journal, I can only conclude that the ultimate decisions to reject were based on factors other than their competence and significance with respect to their circumscribed area of coverage.

While I am sympathetic with the difficulties created for journal editors by the unfortunate combination of too many manuscripts with too little space, I submit that the appropriate solution to this dilemma does not lie in procedures such as those apparently involved in the cases described above. Most important, inconsistency or unannounced changes in policy by an individual journal editor as to what kinds of articles he will or will not accept can only lead to confusion and extra work for all concerned, whether editor, author, or reader. At the very least, it should be the responsibility of APA to coordinate its journals so as to minimize overlap among them, and to insure that at least one journal bears responsibility for each particular type of article that is appropriate for psychological journals. Such is clearly not the case at present, at least with regard to general methodological papers, and probably also other types of papers outside of my direct experience.

Secondly, the use of criteria reflecting popularity with the reading audience, or prominence or status of the author, both of which have been expressed or implied in correspondence concerning these two articles, appear totally inconsistent with the purposes and functions to be served by scientific journals, and

therefore to have no place whatsoever as a basis for the acceptance or rejection of manuscripts by journal editors. Finally, although I know from personal experience that editors sometimes directly forward manuscripts to other journals for which they are deemed more suitable, one of the editors involved has expressly denied that even advice on this point should be the responsibility of a journal editor.

Unless my experience is unique, and I have good reason to believe that it is not, at least the following corrective measures would seem to be in order. First and foremost, each journal should state much more explicitly than is presently the case what types of articles it does or does not accept. To the extent that ambiguous cases still remain (as they undoubtedly will), there should be some sort of autonomous board or committee, preferably *not* consisting of the journal editors themselves, whose responsibility it is to decide in questionable cases which of the APA journals would be most suitable for submission of the article, but without power concerning its actual acceptance or rejection by that journal. Articles where this question might arise could be sent directly to this board by the author or by the editor of the journal to which it was first submitted. Such a procedure would place the responsibility for these decisions at the appropriate level, and leave journal editors free to concentrate on the evaluation of manuscripts solely with respect to their content and significance for their particular subject matter area. Finally, although this ought not to be necessary, perhaps APA journal editors and consulting editors should be reminded that readership popularity as measured by size of circulation, which is inevitably positively correlated with extent of agreement with prevailing viewpoints of the majority, may therefore be incompatible with the presentation of new and different viewpoints which is so vital a part of science. Such factors should not even be considered in the editorial operation of scientific journals, beyond what is absolutely necessary due to those regrettable but inevitable economic considerations which I realize APA must live with.

WILLIAM F. BATTIG
University of Virginia

Lady Bountiful Recognizing and Encouraging Some Talent

While it is difficult to quarrel about how philanthropists should spend their money, the general pervasiveness in society to which the National Merit Scholarship Program aspires suggests that some questions that do not occur in John M. Stalnaker's paper (*Amer. Psychologist*, 16, 513-522) should be given critical consideration.

From the point of view of title, there are some obvious problems with the program as described by Stalnaker. The statement that the Merit Program "... finds able students, wherever they may be, and helps them ... study any curriculum of interest to them ...," is partially contradicted by his later statements. If arbitrary criteria not based on ability or achievement are utilized in order to select persons for scholarships, they can hardly be called Merit Scholarships.

The system described is arbitrarily limited in two important ways that do not involve ability or achievement. The first is through geographic location and the second is on income of family.

1. The allocation of scholarships on a geographic basis has little justification if *merit* is the objective. Conceivably, equalization of standards or support of undeveloped areas and other such objectives might be advanced as important in addition to merit, but they should not be mistaken for merit itself in the sense of achievement and display of ability, and when they are applied they penalize more meritorious persons.

2. It is difficult to think of coming from a low income family as a basis of merit. Certainly in this day and age no one will quarrel with taxation on the ability to pay, but one would have to be foolish not to recognize that people who reach the age of 17 or 18 in any family may wish to develop some degree of autonomy. In essence, the National Merit Scholarship Program provides more opportunity for relative independence for young people from lower than from higher income families. (Of course, the current type of program may have a salutary effect on the economy as forward thinking persons of higher income "live it up" in order to make it possible for their children to become eligible for more favorable scholarships.)

Further, a third limitation appears to be involved to some degree. It becomes difficult to argue that a major factor involved in merit is to choose the "right" field of interest. Such selection has consequences that are peculiar if fields are attracting persons of different background characteristics, and such selection appears to be important in the sponsored scholarships. For example, there is a substantial disproportion of males represented in the report (e.g., 72% in the initial group) that is not explained by the reference to female talent loss on page 522. One would have to ask whether females are less meritorious if they choose the "wrong" fields.

Little experimental work has actually been done on the motivation of persons to achieve or to strive for excellence through reward on the basis of merit. One sociologist has suggested, for example, that the use of

cash prizes instead of scholarships has never been tested to find out whether or not they wouldn't create more competition for high achievement. At least, let us hope, we are not misled by statements such as, "The outstanding record made by those who have graduated is striking." Relationship of this achievement to Merit Scholarships is an inferential tie that is not justified without direct evidence, and testimonials are not sufficient.

Finally, even while recognizing the high motivation and substantial value of philanthropic investments in recognizing and encouraging talent, particularly in arousing public interest, let us hope that such programs do not become palliative substitutes for an objective of equal opportunity through universally available free public higher education.

EDGAR F. BORGATTA
University of Wisconsin

Feedback for the Test Weary

It is no wonder that repetitive, lengthy, and exhausting psychological testing with no feedback produces discouragement and hostility in a job applicant, as reported by Cronbach (*Amer. Psychologist*, 1960, 15, 665-666). Such practices constitute an inefficient use of psychological services, a maladaptive approach to the needs of the individuals and organizations involved, and undesirable publicity for psychology as a whole.

As one step toward dealing with this problem, many consulting firms regularly include a provision for "feedback" to the applicant being assessed. The goal is to

make the assessment experience valuable to the applicant as well as to the employer. The atmosphere is changed from one of testing on a pass-fail basis to one of exploring the possible matches of man and position in terms of the needs of both.

When discussing arrangements with a firm considering our agency's services, we make clear the reasoning and experience that led us to define our role as aiding both applicant and employer. A feedback interview is included as an integral part of the evaluation and is paid for as part of the total fee. We have a further arrangement with some employers whereby they agree to share the cost of a limited number of additional counseling interviews if the applicant desires them, and we feel they may be useful.

With such an arrangement, it is highly unlikely that an applicant would be turned down for a job, or fired, or passed up for promotion without having a fairly clear idea of the psychological variables which contributed to the decision. Such clinical feedback must be presented by a skilled counselor alert to the applicant's ability to accept and integrate information about himself. The difficulty of such feedback does not justify its omission, however.

Our experience has been uniformly favorable with this plan. Candidates and employers have expressed appreciation of the feedback feature. If there is merit in this plan, perhaps its more widespread adoption would reduce the incident of cases such as that reported by Cronbach.

THOMAS C. GREENING
*Psychological Service Associates
Los Angeles, California*

Psychology in the News

Eyeballing

The following is reprinted in its entirety from *Newsweek*:

When an obnoxious commercial flashes on the screen, one tiny consolation has been that it might not be selling any goods to anyone. "Maybe the whole country is disgusted," you could say to yourself. "Maybe the company will be bankrupt by next Tuesday." You were almost always wrong, but you could say it.

Now even this small pleasure is being taken away. McCann-Erickson, an ad agency of stern and rock-bound purpose, has found what it believes to be a foolproof method of pre-testing commercials to see which ones will work and which ones will not. In a special study a University of Chicago psychologist named Dr. Eckhard Hess persuaded hundreds of subjects to stick their heads in a black box. While they were thus fixed, Dr. Hess ran hundreds of objects in front of their eyes, from watch bands to photos of naked ladies. Meanwhile, he trained an infra-red camera on the subjects' eyes. When the object caused pleasure, the pupils of the eyeballs dilated! When the object caused displeasure or disappointment, the pupils contracted!

Soon, viewers, every single commercial to come out of McCann-Erickson will have been eyeball-tested. You may still find some repulsive, but you will have to live with the knowledge that all over the country pupils are dilating wildly, and that the product in question is sure to sell like gumdrops.

* * *

NEA and A-OK . . . Objective Tests Are Here to Stay

A report written by a committee representing several national educational associations called "Testing, Testing, Testing" has been sent to 40,000 school superintendents and principals, criticizing excessive use of standardized tests. The *New York Times* of February 19 gave more than a column to the story, with the heading that it was "A nationwide revolt by school systems against the 'excessive testing' of prospective college students."

The *Times* reported further:

The report, which did not mention testing programs by names, was conducted by a committee of the American Association of School Administrators, the Council of Chief State School Officers and the National Association of Secondary School Administrators.

They urged high schools to "refuse to participate in nationally sponsored tests unless those tests can

be demonstrated to have value commensurate with the effort, money, time and emotional strain involved."

There are now more than twenty national testing programs in addition to those on the state level, such as the Regents examinations in New York. Among the largest national programs are the College Entrance Examination Board and National Merit Scholarship tests.

The committee said that it would be unthinkable to teach without testing. But it asked a lot of questions; for example:

Is it reasonable that a test should be taken by 800,000 pupils (consuming the equivalent of nearly 400,000 pupil school days) to determine 10,000 finalists who will compete to determine 1,000 winners?

The story made a nice background at the Atlantic City meeting of educators, for the next day's report from Project TALENT, in which John Flanagan, John Dailey, and others would discuss some results of their program which had given a two-day test battery to approximately half a million high school students.

Yet it turned out the background for their report was even more spectacular.

On *that* day, Glenn went around the world three times.

Would you care to judge as to whether TALENT was reported in the papers at all? Judge carefully. Do not guess or give up. This is a test. Read on.

SPECIAL NOTE:

Here is the answer to the test question about Project TALENT, the question at the end of the item above, headlined "NEA and A-OK":

Answer: Despite Mercury, psychology made the news that day. For example, Terry Ferrer, education reporter of the *Herald-Tribune*, did a story under a two-column headline, and a digest of it appeared on the *Tribune's* front page. This is better than Theodore Roosevelt did with a trust-busting speech on which he worked very hard. He gave it the day of the San Francisco earthquake—and the rest is history.

Astronaut Behavior

Science Service sought out Robert B. Voas when Colonel Glenn had made his successful trip.

Voas has been psychologist and training officer for the astronauts, and Lillian Levy wrote columns on her interview with him about the human factors of Project Mercury.

Voas described the seven as "superbly well-adjusted" and said that in intelligence, training, and maturity, each one of them was "a man in a million."

The psychologist said that each of the seven expected to get his turn and "if any one did not get a chance to fly, this would be a bitter disappointment. Nothing could be more discouraging to any of these men than not getting into space."

He also said their previous military training and test pilot experience had prepared them for the nature of their hazardous occupation and they all accepted the idea that space flight might involve injury or death. "If a fatal accident should occur in a space try, the reaction would be increased determination to get on with the job," he said.

Voas discussed the interests of each of the seven, and he said that Colonel Glenn is the most concerned of all the seven with the broad political as well as scientific implications of the space program. The psychologist, according to Levy, characterized Glenn as extremely aggressive, tremendously hard-driving and hard-working. "I would not want to compete with John on anything, tiddly-winks included, if he wanted to win," said Voas.

* * *

Psigs of the Times

The ABC-TV network show, "Meet the Professor," on February 25 featured a motion picture interview with Carl G. Jung, and also the appearance of psychologist Richard I. Evans who had made the Jung film. It will be remembered that Evans produced the first course for college credit on educational TV. In Houston he appears on TV regularly, every other Monday, to discuss psychology.

* * *

The Associated Press listened closely to Richard S. Crutchfield at the "Symposium on Control of the Mind," in San Francisco, and sent around the world the story that the American educational system falls

"far short in developing the capacity for independent thinking." The AP said that the University of California's Institute of Personality Assessment and Research had also brought out "that Army officers were the most conforming and research scientists the most independent thinkers."

* * *

Further about Mercury, we liked the line in *Science*, describing the shape of the capsule . . . "a truncated cone with a short cylinder attached at the point of truncation. Less pedantically, it could be said to resemble a cathode ray tube."

Less pedantically than that some people can hardly get—and yet how would *you* describe it? Our hunch is that from now on it will be simpler to say "Well . . . a thing shaped like the astronaut's capsule . . ."

* * *

Science on March 2 editorialized against Senator Byrd's attack on the grant to the University of Wisconsin's program of primate research.

* * *

The Pharmaceutical Manufacturers Association publishes a newsletter, *Medicine at Work*, which in March published several lively news features dealing with hypnosis. Sharp and Dohme and Ciba have lately sponsored meetings and symposia on the subject.

Like many such newsletters, this one quoted anonymous "psychiatrists" or "psychology lecturers," so we cannot know whether the two following items really occurred precisely as described. Psychologist item: a man comes up after a lecture, points to a girl in the audience, and confides, "I've been hypnotizing her for a year; for \$50 I'll transfer control to you." Psychiatrist quote: "Hypnosis is a *folie à deux*, with the hypnotist being by far the sicker of the two."

These items are typical of a number in this particular letter, although there is a page devoted to interviewing Harold Rosen, M. Ralph Kaufman, and Leo H. Bartemeier, all MDs.

—MICHAEL AMRINE

Psychology in the States

Soundings of Soundings-Off

Palm Beach, Fla., Feb. 25 (AP)—President Kennedy today named 12 educators and scientists to membership on a presidential committee on the National Medal of Science. Frederick Seitz, chairman of the physics department at the University of Illinois at Urbana, was appointed chairman. . . .

Other members of the committee, and their special fields, are:

. . . Kenneth E. Clark, 37 (psychology), dean of College of Arts and Sciences, University of Colorado, Boulder, Col. . . .

Maybe the dean was as much flattered at being called 37 (which he isn't) as at being honored (which he is). The Board of Professional Affairs, on the other hand, is as much flattered at being privileged to count him a BPA member (which he is) as at seeing 11 physical scientists joined by one of the behavioral kind (which he is too).

But while the Clarks give their good performance on the legitimate stage, many a drama is being produced in the professional wing, if that it be. The following we find in some of the neighborhood houses.

Peace. If state and local psychological associations were only recently being accused of not seeing beyond certification bills and relations with psychiatry, they may soon be blamed for concentrating on the space race, the coming of cybernation, and India's election returns. However one feels about it, horizons are widening.

The San Fernando Valley Psychological Association, we learn, has recently mailed an open letter to President Kennedy, the concluding paragraph of which reads:

Finally, Mr. President, we wish to pledge to you our support of those policies which you aptly nicknamed "the peace race" and to volunteer our services in whatever way desired to promote alternatives to being "Red" or "Dead." It is unthinkable to us that conquest, war, or appeasement are the only solutions to the national and international problems we now face.

On the other coast, the Nassau County Psychological Association will listen at its March meeting to two speakers—one from the Scientists' Committee for Radiation Information, the second a psychol-

ogist discussing the shelter program and the armament race. We read:

The [NCPA] Board believes that psychologists have a unique contribution to make toward helping the public become informed about human behavior in relation to world tensions. Finding positive solutions to the world issues we face will require new and creative approaches. Our psychology's historical roots in philosophy, physics, and physiology may equip us uniquely for the role of interpreting and approaching these problems in the light of the specific knowledge we have about human behavior.

Pausing for a moment, the newsletter adds:

there is a question of whether NCPA should take a public position on these matters. Any action must be the result of deliberation in the light of the best information. The membership will be polled after this meeting has taken place.

Across the river, in New York City itself, one finds not one but two groups and three possible names for one launched by Stanley Zuckerman last summer—"Psychologists for Peace," "Behavioral Scientists for Peace," or "Social Scientists for Peace." Discussions have been neither academic nor private, and meetings at Yeshiva University have drawn packed houses. Meanwhile other NYSPA members have visibly demonstrated for peace at the United Nations Plaza, and a Committee on Peace is at work drafting position papers for the state association.

For the full and current picture of what APA itself is doing while this world turns and the Colonel orbits, we refer the reader to the January issue of this journal. There the APA Committee on Psychology in National and International Affairs, under the chairmanship of President-elect Charles E. Osgood, has given an account of its stewardship.

More or Less Peace. With psychologists speaking many languages in many quarters, it is not astounding that some noise inevitably gets into the system. The earthy, by now ubiquitous, subject of insurance coverage for psychological services is a case in point. Experimental and social psychologists may see it as less than crucial; for clinical psychologists, and especially private practitioners, it becomes a pretty gritty issue. APA, BPA, committees, and ad hoc committees have been hacking

away at it; state and local psychological associations have followed suit, often with vigor and inventiveness; the National Clinical Liaison Committee vows to keep prodding the Board of Professional Affairs on this and kindred issues. More recently, the newsletter of the New York Society of Clinical Psychologists has carried an article recounting the efforts of two psychologists to get the profession correctly perceived by some key people in the insurance enterprise. Whoever wants to make a safe prediction can bet the general problem will be an item on the agenda of BPA at its spring meeting.

Harold Lindner and Joseph B. Margolin did, indeed, rise to the occasion in making and taking an opportunity to provide some top Federal officials with a three-dimensional view of who psychologists are, what they do, and how all of this makes sense in terms of what the community needs. Whether they are called way-pavers, ground-breakers, or trail-blazers, the two APA members don't much care. They do care whether APA takes up the cudgels. (Not altogether coincidentally, we expect, a resolution has already been received from the Essex County Society of Clinical Psychologists in Private Practice that the central office staff include a psychologist whose primary responsibility be to watch over insurance matters specifically and matters like them.)

Comes now the newsletter of the California State Psychological Association which contains what seems a most reasoned and reasonable statement of the problem. Under the chairmanship of Rogers H. Wright (coincidentally also a member of the ad hoc committee of BPA), the CSPA Insurance Committee enjoins not only APA but state associations and, yes, psychologists generally to rise to the challenge of what is essentially a huge problem of public information. At the risk of appearing to quote selectively, it would seem that Wright is right when he says:

The whole concept of underwriting the costs of psychotherapy by insurance is a relatively new and complex innovation in the insurance business, an innovation which is complicated by many factors.

And he seems broad-gauged when he says:

Procuring remuneration for psychologists under the provisions of Health and Accident Insurance Plans is an extremely broad and complex problem which involves many 'realities' above and beyond the fact that applied psychology is a relative newcomer to the mental health field.

And specific when he says his CSPA Insurance Committee:

would like to see APA assume responsibility for dealing with the insurance issue at a national level in such areas as would be appropriate (i.e., the Federal employees' medical plan) by encouraging the establishment of an active Insurance Committee in every state, and by providing the means and financial support for a national apparatus to coordinate the activities of such committees.

The above may help reassure those, who feel psychology should tend its own store too, that members do talk at less than interplanetary levels. Having so indicated, we continue with the countdown on projects on the launching pad elsewhere.

One Takes One's Stances. Maybe it's spring; signs of life are everywhere. But spring or no, the New York State Psychological Association is given to speaking out on whatever its sense of freedom suggests needs talking to at the moment. Insurance, yes. Broader social issues, definitely.

We have recently come by a 45-page mimeographed opus representing a position statement, in near-final form, of the New York State Psychological Association on the Final Report of the Joint Commission on Mental Illness and Health, prepared for the NYSPA Board of Directors by an ad hoc committee under the chairmanship of Allen V. Williams, Jr. The committee has obviously done its homework, and if the NYSPA members haven't, cramming will now be easy. The working paper reviews what the Joint Commission said and did, retraces its steps to come back to individual JCMIH recommendations, agrees, supplements, or, in some cases, dissents.

The general position recommended by Williams et al. to the NYSPA Board:

In the opinion of the New York State Psychological Association the work of the Joint Commission represents an extraordinary achievement in the mental health field. . . . While we do not accept the total emphasis placed by the Commission on the mental hospital systems and the problem of the hospitalized psychotic as the target for action, the broad and liberal principles embodied in the Report deserve the active support of all members of the New York State Psychological Association.

The reply from the Secretary of Governor Rockefeller to a letter from President Harry V. McNeill offering the cooperation of NYSPA reads in part as follows:

The highly significant work of the individual psychologist—as teacher, researcher, and practitioner—in such settings as universities, community clinics, research centers, correctional facilities, public schools, state hospitals, state schools, and even in independent practice is well known. Your Association's offer to make available the skills and the knowledge of psychologists as a group surely promises to enhance our collective efforts in the important area of mental health.

The Governor will welcome the statement of your ad hoc committee on the Final Report of the Commission and it will be given careful attention.

No matter what the schedule of reinforcement, the New Yorkers continue to speak out. One final word—this time on medical care for the aged:

The New York State Psychological Association recognizes that the inner security as well as the physical health of the aging person is dependent to a large extent on the knowledge that medical care is obtainable whether or not he can afford it.

In view of the above, the New York State Psychological Association recommends that the Congress of the United States pass the Administration's bill providing medical care for the aged through the Social Security system, thus guaranteeing such care as an individual's unquestioned right in his later years.

Meanwhile, back at the Capitol, Senator McNamara and his Special Committee on Aging give thought to still further issues. For in Part 1 of the Hearings before the Committee (1st session, 87th Congress) we read about psychologists and restoration centers as follows:

The major problems of veterans in the restoration center program will be largely psychosocial. They will deal with such factors as motivation, unlearning and relearning. . . . The psychology of individual differences will need to be one wrapped around the concept of capacitation insofar as possible, in serving these individuals through all needed areas of physical, educational, social, and industrial action. . . . Behavior problems will have to be solved by the behavior—the habilitant—with the help of team members in effecting change in perceptual patterns, value systems, and/or other types of relearning. Wherever behavior is involved, there are complexities of motivation, attitudes, aspiration, tolerance levels.

If that can be said better—even in an experimental psychology text—then let him who is without sin cast the first stone.

Of Heresy, Pure and Applied. This month's column seems to have wrapped itself around the thesis that in the chronicles of state association affairs one is likely to find more than one bargained

for. Stereotypes have a way of failing to prove out once data move in, and doubtless members of any APA-divisional faith have some untested notions about what those on the other side of psychology's tracks are up to.

In a recent newsletter of the Los Angeles Society of Clinical Psychologists, Bertram R. Forer, whose name is pronounced foray, made one—across the tracks and back. He was discussing "Psychologists: Clinical and Academic," a topic on which he had held forth in a symposium at the APA convention, and he said in passing:

I should like to take the somewhat extreme stand that the experimental attitude can be an impediment to performing psychotherapy. I suspect that some of the character traits which motivate and substantiate experimental research can be and often are used in opposition to the search for knowledge and that research is too frequently followed with ingenuity to avoid learning anything of importance. There is room for much research on the dynamics of research activities. Clinical knowledge is personal and idiosyncratic and it has to be, even though it be motivated by the same kind of curiosity that impels the experimentalist.

Clinician Forer seated himself next to his couch and continued:

The role of experimenter bias in research may express the same kind of unconscious and inevitable personal involvement in research as countertransference does in psychotherapy. Both groups seek objectivity as a value and both with varied success. The significant part of both science and practice seems to me to be the creative individual extrapolations which provide the new clinical or research hypotheses to be tested. The two sources of knowledge, from testing research hypotheses and exploring interactions with patients, are different and supplementary and ought not be confused. Nor can it be said honestly that either is better than the other. The relative sterility of psychology in the past which has led to some disenchantment on the part of clinical psychologists has been due to the use of scientific method as a defense against anxiety rather than as one of several tools for understanding.

The Los Angeles Society of Clinical Psychologists has, we understand, recently voted a raise in annual dues to \$40. If someone were to suggest a fancy psychodynamic connection with the remarks of the good doctor above, that would be *his* problem.

—ELIOT H. RODNICK

Chairman

Board of Professional Affairs

ERASMUS L. HOCH

Administrative Officer

State and Professional Affairs

Notes and News

President John F. Kennedy has created the President's Panel on Mental Retardation, commissioned to develop a national coordinated program of research and services and to find new approaches to the solution of the many basic problems in this field. Among the 27 panel members are seven APA members: **Leonard S. Cottrell, Jr.**, **Lloyd M. Dunn**, **Nicholas Hobbs**, **William P. Hurder**, **Darrell J. Mase**, **Anne M. Ritter**, and **Harold W. Stevenson**.

The deaths of the following members have been reported:

Margaret Strasser Block, February 4, 1962
David P. Boder, December, 1961
Richard C. Cowden, January 24, 1962
Ernest V. Estensen, February 7, 1962
Robert E. Farley, February, 1962
Frederick Gehlmann, February 3, 1962
C. V. Hudgins, January 30, 1962

Agnes Scott College in **Decatur, Georgia**, announces that **Lee Copple**, formerly of **Vanderbilt University**, has joined the **Psychology Department**.

Robert Apostol, formerly with the **Guidance Bureau** at the **University of Miami**, has accepted a position as **Director of the University Testing and Counseling Service** at the **University of Maine, Orono**.

Lenin A. Baier, formerly **Associate Professor of Psychology** at the **University of New Hampshire**, has accepted the position of **Lecturer on Mental Health** at **Harvard University**.

John R. Barry, formerly at the **University of Pittsburgh**, is now at the **University of Florida-Gainesville** as **Research Coordinator** in the **College of Health Related Services** and **Professor of Psychology** in the **College of Liberal Arts and Sciences**.

Robert R. Benson has been appointed **Chief of Court Psychological Services**, **Montgomery County Juvenile Court**, **Dayton, Ohio**.

Walter V. Clarke Associates announces that **Gerald A. Reynolds**, formerly with the **Boeing Company, Wichita**, has accepted the position as **Regional Associate in charge** of their **Charlotte, North Carolina office**, and that **William T. Gallagher** is now **Regional Associate** of their **Rochester, New York office**.

Columbia Records announces that **Clifford J. Benfield** has been appointed **Director of Management Services**.

Kenneth Crain has been appointed **Supervisor of the Behavioral Sciences Unit, Life Sciences Group**, in the **Research and Development Division of North American Aviation (Los Angeles Division)**.

Gordon F. Derner has been elected **Fellow of the New York Academy of Sciences**.

Stanley Deutsch, formerly with the **Life Sciences Section**, has taken a position as **Chief of the Human Factors Staff** in the **Systems Engineering and Technical Direction of the Skybolt Systems Division, Douglas Aircraft Company, Santa Monica, California**.

Milton Levine has been appointed **Vocational Rehabilitation Counselor of the Federation of the Handicapped's Pre-Vocational Project for Homebound Adolescents**, **New York City**.

George Fry and Associates has appointed **Benjamin S. Alimena** as a **staff psychologist**.

Albert F. Grau has been appointed **Associate Professor of Psychology and Acting Chairman of Psychology and Sociology** at **Laurentian University of Sudbury, Sudbury, Canada**.

Gujarat University of Ahmedabad, India announces that **Pandharinath H. Prabhu**, at present **Fulbright Visiting Professor** at the **Pennsylvania State University**, has been elected **President of the Psychology and Educational Sciences Section of the Golden Jubilee Session of the Indian Science Congress Association** to be held in **January, 1963** in **New Delhi, India**.

Harvard University has announced that **Carl Pfaffman** will be Visiting Professor of Psychology during the academic year 1962-63.

The Human Ecology Fund announces the appointment of **David A. Rhodes** as Executive Director.

The Institute for the Crippled and Disabled has announced that **David H. Powell**, formerly with the VA Regional Office in New York City, has been appointed Staff Psychologist in their Social Adjustment Service.

Lafayette College has appointed **Howard F. Gallup** as Associate Professor of Psychology as of September 1, 1962.

Lessing A. Kahn, formerly with the Office of the Secretary of Defense, Weapons Systems Evaluation Group, is now Senior Analyst with the Arms Control and Disarmament Agency, Weapons Evaluation and Control Bureau.

Dell Lebo has been appointed Chief Psychologist of the Child Guidance and Speech Correction Clinic at Jacksonville, Florida.

The following have recently joined the Faculty of the Department of Psychology and Social Anthropology at the University of Louisville:

Samuel C. Fulkerson, Associate Professor and Director of Clinical Training

Emerson Foulke, Assistant Professor

Richard P. Smith, Assistant Professor

Louise B. Miller, Assistant Professor

Raymond A. Wilkie, Assistant Professor

Minnesota's Community Mental Health Services announces the following appointments:

Donald H. DeKrey, formerly of the Bemidji State College, to the Upper Mississippi Mental Health Center at Bemidji, January 1962

Herbert Dörken, formerly of the Community Mental Health Services of the State of Minnesota, to the Range Mental Health Center at Virginia, March, 1962

William Hunter, formerly of the Cooperative Association for Special Education at Wheaton, Illinois, to the Range Mental Health Center at Virginia, July, 1962

Harold C. Peters, formerly at the Guidance Center of the University of Miami, has accepted a position as Chief Psychologist of the Juvenile Court Branch Clinic of the Dade County (Florida) Child Guidance Clinic.

Allyn F. Roberts, formerly of the Psychology Department at Mendota State Hospital, is now Director of Clinical Services for Midwestern Psychological Services in Madison, Wisconsin.

Edwin S. Shneidman, on leave from the Los Angeles Suicide Prevention Center, VA Central Research Unit, and the University of Southern California, will spend the academic year 1961-62 at Harvard University; he will study the psychologies of suicide on a USPHA Special Research Fellowship.

The Stanford Medical Center of Palo Alto has announced the appointment of **Seymour Levine**, of the Institute of Psychiatry at Maudsley Hospital, London, England, as Associate Professor of Psychology in the Department of Psychiatry, as of April 1.

The Southern Regional Education Board, Atlanta, Georgia, has announced the appointment of **Charles D. Barnett**, formerly with the Austin (Texas) State School, as a project evaluator for the Board; the appointment begins March 1.

Richard W. Seaton, formerly with the Quartermaster Food and Container Institute for the Armed Forces, Chicago, has joined the Stanford Research Institute's facility at the Combat Development Experimentation Center, Fort Ord, California as Research Social Psychologist.

The University of Oregon Medical School has announced the appointment of **Frederick H. Kanfer**, formerly of Purdue University, as Professor of Medical Psychology in the Department of Medical Psychology. **Judson S. Brown** has also been appointed Professor of Medical Psychology in the same department.

The Oregon Psychological Association announces their roster for the Board of Examiners for the calendar year 1962:

David S. Brody

Joseph D. Matarazzo, *Chairman*

Charles F. Warnath

Arthur N. Wiens, *Secretary-Treasurer*

The following organizations have announced their rosters of officers:

International Council of Psychologists

President: **Josephine Ross**

Vice-President: **Leah Gold Fein**

Secretary: Hilde Groth
Treasurer: Helen Driver

San Diego County Psychological Association

President: Edmund C. Dudek
President-elect: Verda T. Heisler
Secretary: Beverly F. Davenport
Treasurer: Paul Bruce

Society for Client-Centered Counseling

President: Samuel Tenenbaum
Secretary: Alexander Bassin
Treasurer: Manny Sternlicht
Membership Director: Norman Lifton
Delegate to Executive Committee: Samuel Buchholtz

The Secretary of Division 16 is now William Itkin, 6724 N. Seeley Ave., Chicago 45, Illinois.

Harold Yuker (1961-1962) replaces Beatrice A. Wright as a Member-at-Large of Division 22's Executive Committee.

The Carnegie Corporation has awarded the following grants:

Dartmouth College, a three-year grant of \$126,000 which will enable the college to explore new methods of organizing subject matter in a variety of fields for self-teaching textbooks and other programed learning devices. Edward J. Green will be one of the directors of the project.

Northwestern University, a grant of \$250,000 to find out what factors cause hostility between groups and how those hostilities may be diminished. The University will engage in a five-year study of intercultural relations. Donald T. Campbell will be a co-director of the program.

Grinnell College has been awarded a grant of \$20,750 by the National Science Foundation to support the Grinnell Behavior Science Institute, a summer science training program in experimental psychology for high-ability secondary school students. The Institute will be held at Grinnell from June 11 to July 28, and the curriculum will consist of lecture and laboratory work in The Experimental Analysis of Behavior, Philosophy of Science and Experimental Methodology, and Statistical Analysis. Participation will be limited to exceptional students, particularly in the sciences and mathematics. Eligibility: completion of the junior year but not the senior year in high school by June of 1962. Deadline for applications is April 15, 1962. For further details, write to Neil D. Kent, Department of Psychology, Grinnell College, Grinnell, Iowa.

Educational institutions may apply for grants from the Office of Vocational Rehabilitation to

establish, expand, or improve their training programs for training of psychologists in rehabilitation or for training of rehabilitation counselors. For further information write to: Assistant Director, Research and Training, Office of Vocational Rehabilitation, Department of Health, Education, and Welfare, 330 Independence Avenue, S.W., Washington 25, D. C.

The **Childhood Schizophrenia Research Center** of the Indiana University Medical Center is offering a summer research fellowship to a qualified psychology graduate student who wishes to devote a summer to an original study of any aspect of childhood schizophrenia or early infantile autism. The fellowship will cover the period from approximately June to September, and pays a stipend of \$400 a month. Applicants will be expected to submit a research prospectus by April 15 for the summer of 1962. The award is on a competitive basis. For further information and application forms, write to Dr. Donald R. Ottinger, Department of Psychiatry, Indiana University Medical Center, Indianapolis 7, Indiana.

The National Science Foundation has awarded grants totaling \$1.6 million to 48 educational institutions to provide research participation programs for college and high school teachers in an effort to strengthen science education at all levels and to advance scientific graduate education through participation in basic research. The cooperating institutions with programs in psychology are as follows:

For college teachers

Florida State University, a grant of \$21,110 for five predoctoral and five postdoctoral teachers; the term is for nine weeks beginning June 11, and preference will be given to teachers from the Southeastern United States. Inquiries and applications should be directed to: Barron B. Scarborough, Department of Psychology, Florida State University, Tallahassee, Florida.

Indiana University, a grant of \$10,600 for two predoctoral and five postdoctoral teachers; the term will be for eight weeks beginning June 13, and preference will go to teachers from the North Central United States. Inquiries and applications should be directed to: Roger W. Russell, Chairman, Department of Psychology, Indiana University, Bloomington, Indiana.

University of Michigan, a grant of \$30,740 for ten postdoctoral teachers; the term will be for ten weeks beginning June 19. Direct applications and inquiries to: Robert L. Isaacson, Department of Psychology, University of Michigan, Ann Arbor, Michigan.

Pennsylvania State University, a grant of \$14,430 for one predoctoral and five postdoctoral teachers; the term is for ten weeks beginning June 12, and preference will be given to teachers from the East and North Central United States. Inquiries and applications should be sent to: William M. Leploy, Department of Psychology, Pennsylvania State University, University Park, Pennsylvania.

For high school teachers

University of California, Berkeley, a grant of \$39,150 for a program including psychology, among other areas, for 25 high school teachers. The term is for eight weeks, beginning July 9. Address inquiries and applications to: Fred E. Dickinson, Director, Forest Products Laboratory, University of California, Berkeley, California.

The National Institute of Mental Health has awarded an \$18,000 grant to **Boston University** for a three-year project for the examination of social conditions affecting communication of schizophrenic patients. **Leonard F. Solomon** will direct the study.

Announcement has been made of the establishment of the **Stanley R. Dean Research Award**, an annual grant of \$2,000 for basic research accomplishment in the behavioral sciences contributing to understanding schizophrenia.

The **Center for the Scientific Study of Suicide** is now accepting applications for positions either as Fellow or as Staff Member under a USPHS project grant, to start September 1, 1962. Fellows ordinarily will be appointed for a one- or two-year period, and may pursue researches on topics relevant to suicide and suicide prevention. Staff members will have an opportunity to work with ongoing clinical research projects on suicide. Candidates must have completed a PhD with major work in clinical psychology, social psychology, sociology, anthropology, or philosophy, or have relevant equivalent training. Salaries open but liberal. Address applications, preferably by July 1, 1962, to: Edwin S. Shneidman, Department of Social Relations, 7 Divinity Avenue, Harvard University, Cambridge 38, Massachusetts.

The **Suicide Prevention Center of Los Angeles** is offering a position as either Psychology Fellow or Psychology Trainee under a USPHS grant for a period of six months to a year, beginning September 1, 1962. Excellent research possibilities will be available to clinical psychologists with the PhD

from a recognized university, or students in the fourth year of graduate work in a clinical psychology program. A stipend of \$6,000 per year, or proportions thereof for part time, is provided. Apply to **Norman L. Farberow**, Co-Project Director, Suicide Prevention Center, Box 31398, Los Angeles, California.

The University of Chicago's Department of Psychology announces two workshop seminars in the **Rorschach Test** for the summer of 1962. Enrollment in each seminar is limited to 50 students. For information and applications, write to: Rorschach Workshops, Department of Psychology, University of Chicago, Chicago 37, Illinois.

Colgate University will present its second **Symposium on Hypnosis**, "Frontiers of Application", April 5-7, 1962. For further information, write to **G. H. Estabrooks**, Colgate University, Hamilton, New York, or call Ham. 660, Ext. 40.

The Department of Psychological Services, **Springfield State Hospital**, announces that the **Virginia Beyer Memorial Lecturer** of 1962 will be **Rollo May**. The topic will be "The Existential Approach to Freedom and Responsibility in Psychotherapy," and the lecture will take place on May 21, 1962 at 8 p.m. in the Auditorium of the Geriatrics Building. For further information, write to **Julian Abrams**, Chief of Psychological Services, Springfield State Hospital, Sykesville, Maryland.

The **Matrix Corporation** of Arlington, Virginia has recently acquired the assets of Courtney and Company of Philadelphia, which will continue as an operating division of Matrix. **Douglas Courtney** has been named Senior Vice President and a member of the Board of Directors of Matrix, and will continue to direct operations of the Philadelphia group.

On February 10, 1962 **Norman M. Molesko** of the Human Reliability Requirements Group of General Dynamics/Astronautics, addressed the San Francisco Psychoanalytic Education Society on "Human Factors/Reliability of Man in Space and on Lunar Missions".

The University of Washington's Department of Psychology and the Puget Sound Psychological Association are among the sponsors for educa-

tional sessions of the 2nd National Northwest Summer Conference, "The Special Child in Century 21," to be held August 26–September 1, 1962 in Seattle. For details write to: Jerome Hellmuth, Executive Secretary, 2nd National Northwest Summer Conference, 113 Madrona Place East, Seattle 2, Washington.

A conference will be held on April 13–14, 1962 at the University of Virginia, Charlottesville, Virginia on the general topic "Conditioning Therapies versus Psychoanalysis: the Challenge in Psychotherapy". W. Horsley Gantt, Joseph Wolpe, Cyril Franks, A. J. Bachrach, and L. J. Reyna are among those who will present papers.

The Committee on Diagnostic Reading Tests announces Work Conferences in Reading for the summer of 1962: July 2–13, Pace College, 41 Park Row, New York City; July 30–August 10, Committee Conference Headquarters, Suburban Hendersonville, North Carolina. For details write to Francis Triggs, Mountain Home, North Carolina.

The International Council of Psychologists (formerly the International Council of Women Psychologists) was incorporated as a nonprofit organization in Connecticut in November, 1961 and now welcomes men psychologists to membership. The society is dedicated to the development of a sound international psychology, calling for facilitating communication and cooperation among psychologists throughout the world. Membership applications are obtainable from Dee Apply at Southern Illinois University, Carbondale, Illinois.

The Society of Psychomycology will hold its initial meeting at New Mexico Highlands University on April 14, 1962. Inquiries concerning the Society may be addressed to Alan H. Roberts, Psychology Department, New Mexico Highlands University, Las Vegas, New Mexico.

The 1962 Annual Workshop in Projective Drawings will be conducted at the New York State Psychiatric Institute, New York City, on July 23–26, from 10 a.m. to 12 noon, and 1 p.m. to 3 p.m. daily.

The National Science Foundation announces closing dates for receipt of basic research proposals: Social Sciences, May 1, 1962; Biological and Med-

ical Sciences, May 15, 1962. Proposals received prior to these dates will be notified of the Foundation's action within four months; those received after these dates will be reviewed following the fall closing dates (Biological and Medical Sciences, September 15, 1962; Social Sciences, October 1, 1962). Address inquiries to the National Science Foundation, Washington 25, D. C.

The Institute for the Crippled and Disabled announces a Workshop on Concepts of Mental Hygiene for Rehabilitation Personnel, June 6–8, 1962. The workshop is sponsored by the Office of Vocational Rehabilitation, and enrollment will be limited to 25 persons. Applications and inquiries should be addressed to: Director of Professional Education, Institute for Crippled and Disabled, 400 First Avenue, New York 10, New York.

The Twenty-fifth Annual Reading Conference will be held at the University of Chicago from June 26 through June 29, 1962. The central theme will be "The Underachiever in Reading." The advance program may be secured about May 1 from H. Alan Robinson, Conference Director, 5835 Kimbark Avenue, Chicago 37, Illinois.

Advanced training in psychoanalytic psychotherapy is available to a few well-qualified clinical psychologists at the William Alanson White Institute of Psychiatry. For bulletin describing the program, address the Registrar of the Institute, 12 East 86th Street, New York 28, New York.

Late in 1961 several APA members visited the Psychometrical Laboratory of the Polish Academy of Sciences in Warsaw. Donald G. Marquis, James G. Miller, Arthur B. Sweney, Paul Horst, John C. Flanagan, and Lyle V. Jones addressed the Laboratory staff and other Polish psychologists on various subjects, and consulted with the staff on various research problems. Other visitors to the Laboratory were Oscar K. Buros, William C. McCord, and William C. Kvaraceus.

An Institute of Dream Research has been established by Calvin Hall to investigate the contents of dreams. For details write to the Institute, Box 1022, Coral Gables 34, Florida.

The Asia Foundation again makes an appeal for donations of books and journals for overseas dis-

tribution under the Books for Asian Students Program. Since its inception seven years ago, the program has distributed over two million books and half a million journals among 5,000 Asian institutions. Government and private institutions in Asia look to Asia Foundation for book and journal aid, and requests have increased. All shipments or questions concerning categories, criteria, shipping, and program details should be addressed to: Books for Asian Students, 21 Drum Street, San Francisco 11, California.

Heterozygous male rats are being offered free in limited quantity to psychologists. The animals are heterozygous for the gene that allows expression of color. Breeding with an albino female produces a litter about half pigmented pups and half albino. Thus strain differences and environmental factors are held constant while only pigmentation varies. The animals will be available through June on a first-come, collect-shipment basis. Write to Robert

B. Lockard, Department of Psychology, University of Wisconsin, Madison, Wisconsin.

The American Psychological Association is again participating with eight other national professional societies in the maintenance of the **National Science Foundation Register of Scientific and Technical Personnel**. A 1962 reporting form and a summary of the 1960 Register results are being mailed to all APA members in April by the APA Central Office. Members are invited to complete the 1962 reporting form and return it as soon as possible. Valid current data is important not only for the National Register, but also for special studies of American psychologists which can now be conducted by the Manpower Resources Division of the Central Office.

Betty Mallinger Goldbloom was listed in error with the group of members whose resignations have been accepted by the Board of Directors.

Convention Calendar

American Psychological Association: August 30-September 5, 1962; St. Louis, Missouri

For information write to:

George S. Speer
American Psychological Association
1333 Sixteenth Street, N.W.
Washington 6, D. C.

Eastern Psychological Association: April 26-28, 1962; Atlantic City, New Jersey

For information write to:

Marvin Iverson
P. O. Box 601
Garden City, New York

Deadline for papers: December 1, 1961

Southeastern Psychological Association: March 29-31, 1962; Louisville, Kentucky

For information write to:

Ray H. Bixler
Department of Psychology
University of Louisville
Louisville 8, Kentucky

Midwestern Psychological Association: May 3-5, 1962; Chicago, Illinois

For information write to:

G. Robert Grice
Department of Psychology
University of Illinois
Urbana, Illinois

Southwestern Psychological Association: April 5-7, 1962; Fort Worth, Texas

For information write to:

Gordon V. Anderson
University of Texas
P. O. Box 8017, University Station
Austin 12, Texas

Rocky Mountain Psychological Association: May 10-12, 1962; Butte, Montana

For information write to:

Thomas C. Burgess
Montana State University
Missoula, Montana

Southern Society for Philosophy and Psychology: April 19-21, 1962; Memphis, Tennessee

For information write to:

Dan R. Kenshalo
Department of Psychology
Florida State University
Tallahassee, Florida

Western Psychological Association: April 19-21, 1962; San Francisco, California

For information write to:

Herbert Naboišek
Division of Psychology
San Francisco State College
San Francisco, California

St. Louis, Missouri, August 30–September 5, 1962

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..... I do not want a hotel reservation

Please check carefully the hotel and rate information shown on opposite page. Be sure to complete all of the information requested below:

Type of accommodation desired: Single Double Twin Dormitory

Hotel preference:

Rate requested: \$.....

1.

2.

3.

When more than one person will occupy the room, please give the following information for all occupants:

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Advanced Registration Forms received after August 1 may not be processed for preregistration

HOTEL AND RATE INFORMATION

SEVENTIETH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

St. Louis, Missouri, August 30-September 5, 1962

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Ambassador				S	S	S D	D	D T	D T	T	T			
*Bel Air Motel						S S	S	S	S D	S D T	D T	D T	D T	T
*Chase Park Plaza					S	S	S	S D	D	D T	D T	D T	T	T
Claridge	S	S	S	S D	D T	D T	D	T		T				
*Congress			S S	S	T				T					
Coronado				S	S	S	S D	D		D T	D T	T	T	
*Diplomat Motel					S	S	S	S D	D	D	T	T	T	
*Pick-Mark Twain				S	S		D	D T	T					
*Sheraton-Jefferson				S		S	S	D T		S D T	T			
*Statler-Hilton				S	S	S	D	S D		D T	D T		T	

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(Typewrite or print in pencil) **APA CONVENTION PLACEMENT OFFICE** **Applicant Number**.....

Name: Sex:

Convention address: Phone:

Permanent address:

APA membership: Yes No

Academic Training: Degree	University	Year
Describe pertinent work history:		
Number of papers presented or published:		

Type of position desired:

Expected salary (optional): Preferred location:

Give a tentative schedule of the time you plan to be available in the Convention Placement Office for interviews:

Aug. 30		Aug. 31		Sept. 1		Sept. 3		Sept. 4		Sept. 5	
A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.

Under which field of specialization do you wish your application to be filed? Check no more than two boxes:

	Clinical
	Counseling
	Educational School

	Developmental
	Experimental
	General
	Physiological

	Human Engineering
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POSITION DESCRIPTION FORM

(Typewrite or print in pencil) APA CONVENTION PLACEMENT OFFICE Position Number.....

- I. Name and Address of Employing Organization:
- II. Position Title (include location if different from I.):
- III. How many openings does this form cover:
- IV. Applicant Requirements:

A. Degree:	1. PhD (EdD)	2. PhD preferred, will consider Master's
	3. Master's	4. Other.....
B. Experience:		
C. Other:		
Sex: Male Female Either		

V. Position requirements:

A. Duties:		
B. Starting Salary (depending on qualifications): Minimum Maximum		
C. Position Begins: Immediately or, give date		

- VI. A. If you wish space to interview applicants, fill in the name of the interviewer and check your tentative interviewing schedule:

Aug. 30		Aug. 31		Sept. 1		Sept. 3		Sept. 4		Sept. 5	
A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.

- B. If you have no plans for interviewing, please indicate the person to whom inquiries are to be sent:

VII. Check one category for listing your position:

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1. I volunteer for a maximum of _____ periods from the ones indicated below.

2. Place the number 1 in each of your two (or more) first-choice times, the number 2 in each of your second-choice times, and the number 3 in each third-choice time. If you are concerned about avoiding time conflicts with programs of interest, consult the convention schedule in the November *American Psychologist* or the official program in the July *American Psychologist*.

	Wed. Aug. 29 Pre- convention	Thurs. Aug. 30	Fri. Aug. 31	Sat. Sept. 1	Sun. Sept. 2	Mon. Sept. 3 Labor Day	Tues. Sept. 4	Wed. Sept. 5
8:45–12:45								
12:45– 4:45								
4:45– 8:15								

Note: There *will* be mealtime breaks for those who serve successive periods on the same day.

3. If you volunteered for a morning session, could you begin at 8:15 A.M. if necessary? Yes ☐ No ☐

4. Check the services you offer:

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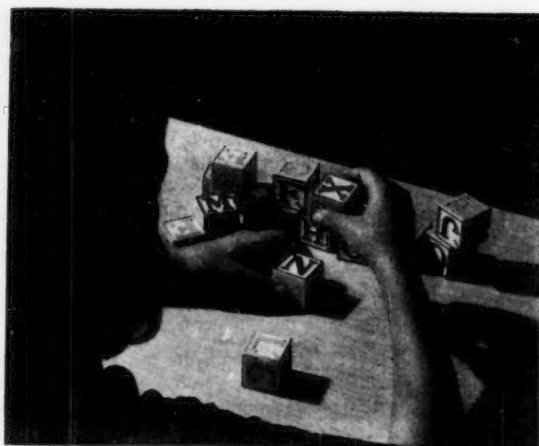
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Position (if *not* a student): _____ Organization: _____

5. Please detach and mail above to: Norman L. Corah, Dept. of Psychology, Washington University, St. Louis 30, Missouri.

6. You will hear from us by August 20th notifying you of your assignment. If you do not, please come to the Volunteer Workers Desk on your arrival at the Convention (Lucas Room, Chase). If a change occurs in your address or availability, please write to Corah (at the above address). Fill out, detach, and save this duplicate slip.

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Foundations and Use in the Behavioral Sciences

by KENNETH R. HAMMOND, *University of Colorado*, and JAMES E. HOUSEHOLDER, *Humboldt State College*

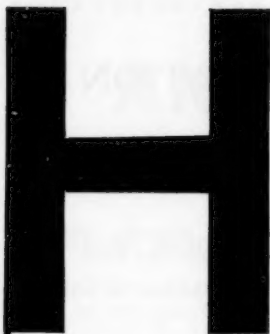
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